HARVARD PSYCHOLOGICAL STUDIES

EDITED BY

HUGO MÜNSTERBERG

Volume II

BOSTON AND NEW YORK
HOUGHTON, MIFFLIN AND COMPANY
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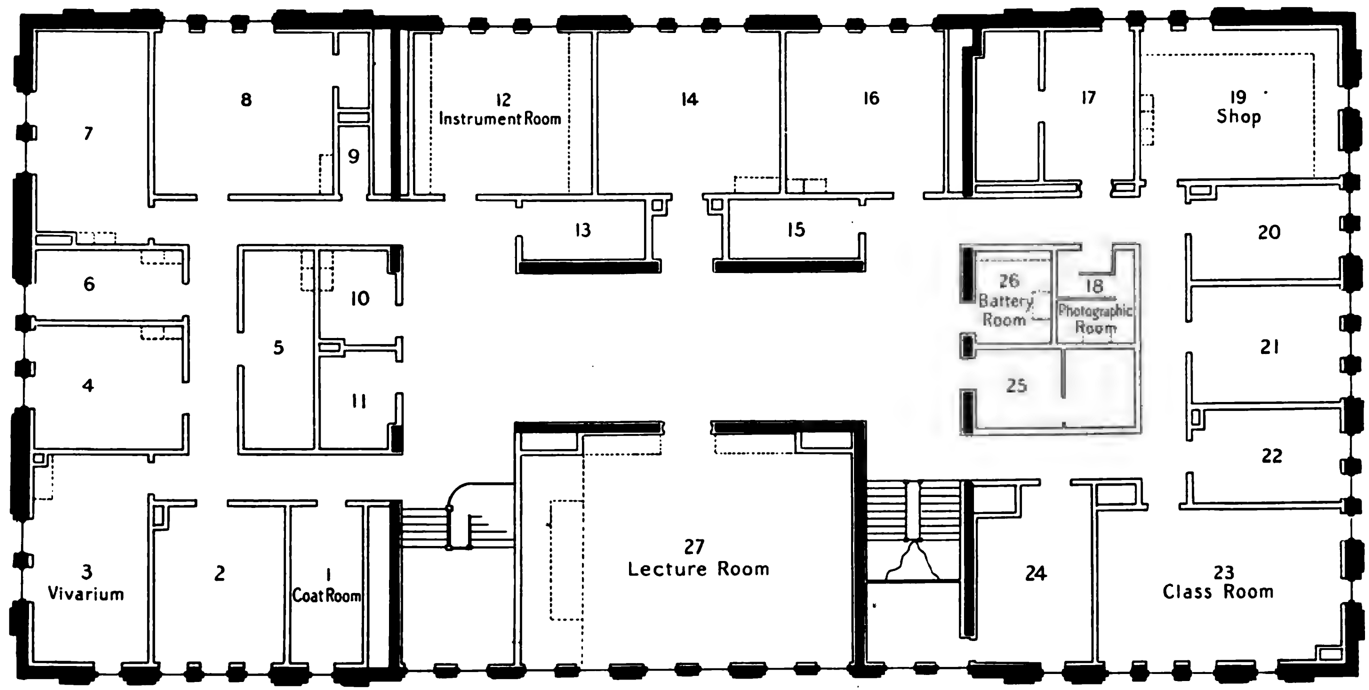


HARVARD PSYCHOLOGICAL LABORATORY

EMERSON HALL

BY HUGO MÜNSTERBERG

I. Experimental Psychology in Harvard

On the 27th of December, 1905, Harvard University opened its new house of philosophy, Emerson Hall. The presence of the American Philosophical and Psychological Associations gave national significance to the completion of this building.

The psychologist will find quarters in all parts of Emerson Hall. The general courses in psychology will be held on the first floor in the large lecture-room, which has nearly four hundred seats; and close by are the psychological seminary-room and smaller lecture-rooms for the advanced psychological courses. On the second floor the psychologist finds his special library as a wing of the large library hall. But the exclusive domain of the psychologist is the third floor, — a psychological laboratory with twenty-five rooms. A large attic hall for laboratory purposes on the fourth floor completes the psychologist's allotment.

The work to be reported in future in the Harvard Psychological Studies will be work done in this new building, and while the researches reported in the following pages were completed in the smaller quarters of the old laboratory, it seems natural that this volume, which appears at this new epoch of our work, should give an account both of our psychological past and of the development and purpose of Emerson Hall.

The Harvard Psychological Laboratory was founded in 1891 by Professor William James, who had introduced some experimental features into his psychological lecture courses for some time before the formal opening of a regular workshop. Professor James started with two large rooms on the second floor of Dane Hall, and secured an excellent equipment, especially for the study of the psychology of the senses. He was assisted by Dr. Herbert Nichols, and at once gathered a number of graduate students for research.

In the following year Professor James withdrew from the experimental work, and the conduct of the laboratory was given over to me. In the years which followed, Dr. Arthur Pierce, Dr. J. E. Lough, and Dr. Robert MacDougall were the assistants until three years ago, when the development of the laboratory demanded a division of the assistant functions; since that time Dr. E. B. Holt has been the assistant for the work in human psychology, while Dr. R. M. Yerkes has had charge of the work in comparative psychology. Since from the first I laid special emphasis on research work, a greater number of small rooms was soon needed. In the year 1893, we divided a part of the adjacent lecture-room into four rooms for special investigations, and two years later the larger of the two original rooms was divided into five. As the lecture-room also was finally made part of the research laboratory, we had at last eleven rooms in Dane Hall. The activities of the laboratory, however, went far beyond the research work. We had regular training-courses in experimental practice, and the lecture courses in human and in comparative psychology drew largely on the resources of our instrument cases. Yet the original investigations absorbed the main energy of the laboratory, and demanded a steady expansion of its apparatus. An illustrated catalogue of the instruments has been published as part of the Harvard Exhibition at the Chicago World's Fair.

The participation of the students has been controlled by a principle which has characterized our Harvard work through all these years, and distinguished it from the methods of most other institutions. I insist that no student shall engage in one investigation only, but that every one who has charge of a special problem shall give to it only half of his working time, while in the other half he is to be subject in four, five, or more investigations by other members of the laboratory. In this way each research is provided with the desirable number of subjects, and all one-sidedness is avoided. Every experimenter thus comes in contact with a large range of problems and gets a fair training in manifold observations, besides the opportunity for concentration on a special research. It is true that this demands a complicated schedule and careful consideration of the special needs of every research, but it gives to the work a certain freshness and vividness, and banishes entirely the depression which is unavoidable whenever a student is for any length of time a passive subject in one psychological enquiry only. In both capacities, as experimenter and as observing subject, only graduate students have been acceptable. In this way about *one hundred investigations* on human psychology

have been carried on, for most of which I have proposed the problems and the special lines of work, taking care that the research of succeeding years and of succeeding generations of graduate students should show a certain internal continuity. Whenever the results seemed fit for publication, the papers have been published under the names of the students who had the responsibility for the conduct of the experiments. Until three years ago the publication was scattered; most of the papers, however, appeared in the Psychological Review. The Harvard Psychological Studies, beginning in 1903, are to gather the bulk of our material, although not a few of the researches of recent years have been published in other places.

The laboratory has always sought to avoid one-sidedness, and this the more as it was my special aim to adjust the selection of topics to the personal equations of the students, many of whom came with the special interests of the physician, the zoölogist, the artist, the pedagogue, and so on. My own special interests may have emphasized those problems which refer to the motor functions and their relations to attention, apperception, space-sense, time-sense, feeling, etc. At the same time I have tried to develop the psychological-æsthetic work, which has become more and more a special branch of our laboratory, and there has been no year in which I have not insisted on some investigations in the fields of association, memory, and educational psychology. On the other hand, in a happy supplementation of interests, Dr. Holt has emphasized the physiological psychology of the senses, and Dr. Yerkes has quickly developed a most efficient experimental department of animal psychology.

As the work thus became more manifold, the old quarters in Dane Hall appeared less and less sufficient. And yet this laboratory development has been merely parallel to the growth of general philosophical studies in the whole University. The demand for a new hall, exclusively devoted to philosophy, was thus suggested from many sides. The idea of linking it with the name of Ralph Waldo Emerson has been for years a cherished plan of Professor Palmer.

An especially appropriate time for the realization of such a plan came in the approach of the hundredth anniversary of Emerson's birthday. Almost two years before this date the Department took the first steps in seeking to interest the members of the Visiting Committee for the collection of the necessary funds. This Committee, consisting of Mr. G. B. Dorr, chairman, Mr. R. H. Dana, Dr. R. Cabot, Mr. J. Lee, Mr. D. Ward, and Mr. R. C. Robbins, showed not only warm interest, but lent itself to the furtherance of the plans

with such an energy and devotion that the Philosophical Department owes to these friends of philosophy in Harvard the most lasting gratitude. Various means were taken by the Committee and by the Department to stir the interest of the public, and soon the gifts began to come in, gifts of which some were clearly given from sympathy with the work of the Philosophical Department, some evidently in memory of Emerson. The original plans of the architect called for $150,000 for the building. When, on the 25th of May, 1903, the hundredth anniversary of Emerson's birthday was celebrated, the University had contributions amounting to more than this sum, and given by one hundred and seventy persons.

It was soon found, however, that this sum was inadequate; yet we never asked in vain. Additional gifts came in for the building fund, just as later the generosity of several friends furnished the building with a handsome equipment and the laboratory with new instruments. Mr. R. C. Robbins gave the books for a philosophical library to be placed in the new Hall.

The architect chosen was Mr. Guy Lowell, who has had to labor under the difficulties involved in the fact that the best and quietest available place was on Quincy Street opposite Robinson Hall. This spot demanded that the new building be harmonized with Robinson and Sever Halls, two structures most unlike in their architectural style. There was not even the possibility of making it a companion to Robinson Hall, since the latter has but two stories, while it was evident that Emerson Hall needed three stories. The plan finally accepted, a Greek, brick building with brick columns and rich limestone trimmings, provided for the work of the whole Philosophical Division with the exception of education. The Education Department, with its large library, will soon need a whole building of its own, and has thus had no interest in being housed under the roof of Emerson Hall. On the other hand, the building was to give full space to that part of our Philosophical Division which now forms, like education, an administrative unity, — the Department of Social Ethics. A special library, museum rooms, etc., for social ethics were planned for the second floor by the munificence of an anonymous benefactor. Altogether we have six large lecture-rooms, two library halls, two collection-rooms, a department-room, a seminary-room, two studies and conference-rooms, twenty-five laboratory-rooms, all connected by very spacious, well-lighted halls and broad, imposing stairways. Surely never before in the history of scholarship has such a stately house been built for philosophy. And while the nature of the work

is certainly not determined by the luxury of stone and carved wood, teachers and students alike must feel these superb surroundings as a daily stimulus to their best efforts.

At Christmas, 1905, the building stood ready for use, and Duveneck's bronze statue of Emerson was unveiled in the entrance hall. At the opening meeting, after short dedicatory orations by President Eliot and Dr. Edward Emerson, a real exchange of ideas in a joint debate of the Philosophical and Psychological Associations was substituted for the usual formal exercises. The question debated was suggested by the fact that Emerson Hall was to house the psychological laboratory. Does psychology really belong to philosophy or rather to the natural sciences? As the representative of Harvard, it was my part to open the debate and to characterize the attitude of the Harvard laboratory.

My remarks on that occasion may thus serve as the most direct introduction to our work. They are printed here, together with a short sketch of the equipment of the laboratory. I venture to add also two other papers, one of which points to the administrative, the other and longer one to the philosophical background of Emerson Hall. Inasmuch as I was Chairman of the Philosophical Department throughout the five years in which the plan for Emerson Hall was growing and became finally realized, it has been my official duty repeatedly to express our hopes and ideals. Thus I had to formulate the wishes of the Department at the outset in a letter to the Visiting Committee, a letter which was used as a circular in asking the public for funds. Two years later when Harvard celebrated the Emerson anniversary, I delivered an address on Emerson as philosopher. This epistemological paper may seem far removed from the interests of the Harvard Psychological Studies, and yet I am glad to print it in this laboratory volume, and thus emphatically to indicate that I for one consider philosophy the true basis for the psychologist.

There follow thus, first, the letter to the Visiting Committee, with which the Emerson Hall movement took its official inception in 1901; secondly, the address delivered at Harvard on the celebration of the Emerson anniversary in May, 1903; thirdly, the paper contributed to the debate of the philosophers at the opening meeting in December, 1905; and, finally, a description of the present status of the laboratory in January, 1906.

II. The Need for Emerson Hall

[The letter addressed to the Visiting Committee of the Overseers of Harvard University, in 1901, reads as follows:]

Gentlemen, — The philosophical work in Harvard has in the last twenty years gone through an inner development which has met with a hearty response alike on the part of the University and of the students. The students have attended the courses in constantly growing numbers, the Governing Boards have provided the Division amply with new teachers, steadily increasing the number of professors, instructors, and assistants. The outer growth of the Division has thus corresponded most fortunately to the internal development, by an harmonious coöperation of the administration, the teachers, and the students of the University. And yet there remains one other factor as an essential condition for the healthy life of the Department, a factor which cannot be provided by the University itself and for which the help must come from without. Our work needs a dignified home where under one roof all the varied philosophical work now carried on at Harvard may be united. The need has been urgently felt for many years, but only with the recent growth has the situation become intolerable. It is therefore the unanimous opinion of the Department that we must ask the public for the funds to build at Harvard a "School of Philosophy," in the interest of the students and of the teachers, in the interest of the Department and of the University, in the interest of culture and of scholarship.

The present work of the Division of Philosophy can be indicated by a few figures. We entered the current year with a teaching-staff of six full professors, two assistant professors, four instructors, two teaching-fellows, and six assistants. The instruction of these twenty men covers the ground of history of philosophy, metaphysics, theory of knowledge, psychology, logic, ethics, æsthetics, philosophy of religion, philosophy of science and sociology. Thirty-two courses have been offered. These courses are grouped in three classes: the introductory courses, intended primarily for Sophomores and Juniors; the systematic and historic courses, planned for Juniors, Seniors, and Graduates; and the research courses for Graduates only. But the students whom we try to reach differ not only with regard to their classes, their corresponding maturity, and their degree of philosophical preparation, but also with regard to the aims and interests for which they elect philosophical studies in the University. The one group seeks in our field liberal education. The fundamental

problems of life and reality, and the historic solutions of them which the great thinkers developed, the values of truth and beauty and morality, the laws of the mental mechanism and of the social consciousness, all these promise and prove to be incomparable agencies for widening the soul and giving to our young men depth, strength, and ideals. Not a few of the students who belong to this group remain loyal to philosophy through three or four years. A second group of students need our courses as preparation for divers scholarly or practical aims. The future lawyer, teacher, physician, minister, scientist, or philanthropist knows that certain courses in ethics or psychology, in education or logic afford the most solid foundations for his later work; there is hardly a course in our Division which is not adjusted to some kind of professional study. The third group finally, naturally the smallest, but to the teachers the most important, consists of those to whom philosophy itself becomes a life's work. The Harvard Department believes that there is nowhere else in this country or abroad such an opportunity for systematic and all-round training for an advanced student of philosophy as is offered here, covering easily a man's full work for six years, advancing from the introductory courses of the Sophomore year to the six seminaries of the graduate years and finally reaching the doctor's thesis in the third year after graduation.

The extent to which the Harvard students make use of these opportunities is to be inferred from the figures which the last Annual Report of the President offers. These refer to the year 1899–1900; the current year will show somewhat the same proportions, perhaps even an increase of graduate work. The figures are necessarily too low, inasmuch as they refer merely to those students who take examinations in the courses and omit those who merely attend the lectures. The attendance in the philosophical courses was last year over one thousand students. They belonged to all parts of the University, 188 Graduates, 210 Seniors, 218 Juniors, 175 Sophomores, 59 Specials, 57 Scientifics, 55 Divinity students, and the rest from the Freshman class, the Law School, and the Medical School. The introductory courses were attended by almost four hundred students, that is, by a number corresponding to the size of the Junior class. As, in spite of natural fluctuations, this figure is pretty constant,—in 1897 reaching its maximum with 427,—it can be said that in Harvard under the system of absolutely free election practically every student who passes through Harvard required of himself at least a year of solid philosophical study.

An even higher interest, however, belongs to the figures which refer to the most advanced courses offered, especially to the courses of research. It has always been the most characteristic feature of the Harvard Philosophical Department to consider the advancement of knowledge as its noblest function. The productive scholarship of the Department is shown by the fact that the last two years alone brought before the public eight compendious scholarly works from members of our Department, besides a large number of smaller contributions to science. To train also in the students this highest scholarly attitude, that of the critical investigator as contrasted with that of the merely receptive hearer of lectures, is thus the natural aim of our most advanced work; it is this spirit which has given to the Department its position in the University and in the whole country. This prevalence of the spirit of research is the reason why, as the Report of the Dean of the Graduate School points out, the Philosophical Department has a larger number of graduate students who have carried on graduate studies elsewhere than any other Department of the University. The table of the Dean which records these migrating graduate students who come to us for advanced work after graduate studies at other universities, is as follows: Mathematics 6, Natural History 7, Political Science 7, Modern Languages 11, Classics 14, History 15, English Literature 16, Philosophy 20. If we consider the whole advanced work of the University, that is the totality of those courses which are announced as "primarily for Graduates," we find that the following number of graduate students, including the graduate members of the professional schools, have taken part: Classics 103, Philosophy 96, English 75, German 61, History and Government 52, Romance Languages 45, Mathematics 39, Economics 23, Chemistry 21, in the other departments less than twenty. But this situation turns still more strongly in favor of philosophy as soon as we consider the technical research courses, those which in the language of the catalogue are known as the 20-courses, and omit those graduate courses which are essentially lecture courses. In these research courses the number of Graduate Students is: Philosophy 71, History and Government 34, Chemistry 13, Zoölogy 12, Geology 10, and in the other departments less than ten.

These few figures may be sufficient to indicate not only the extent of the Department and its influence, but above all the harmonious character of this development. The most elementary courses, the solid routine courses, and the most advanced courses, show equal signs of growth and progress, and the whole work with its many side

branches remains a well-connected unity with a clear systematic plan. All this must be understood before one can appreciate the striking contrast between the work and the workshop. It is of course easy to say at once that the truth of a metaphysical thought does not depend upon the room in which it is taught, and that the philosopher is not, like a physicist or chemist, dependent upon outer equipments. Yet, this is but half true, and the half of the statement which is false is of great importance.

The dependence upon outer conditions is perhaps clearest in the case of psychology, which has been for the last twenty-five years an objective science with all the paraphernalia of an experimental study: the psychologist of to-day needs a well-equipped laboratory no less than the physicist. Harvard has given the fullest acknowledgment to this modern demand and has spent large sums to provide the University with the instruments of an excellent psychological laboratory; the one thing which we miss is room, simply elbow-room. Our apparatus is crowded in the upper story of Dane Hall, and even that small story must give its largest room for the lectures of other departments and another room to a philosophical reading-room. The space which remains for the psychological work is so absolutely out of proportion to the amount of work going on that the problem how to bring all the men into those few rooms has become the most difficult of all our laboratory problems. During the current year, besides the training-courses, twenty-three men are engaged there in original research, each one with a special investigation and each one anxious to devote as much time as possible to his research; only the most complicated adjustment makes it possible at all, and yet the mutual disturbance, the necessity of passing through rooms in which other men are working, and of stopping the work when other men need the place interfere every day with the success of the instruction. A mechanical workshop is an urgent need of our laboratory, and yet we cannot afford the room; and while the only desirable arrangement would be to have the psychological lectures in the same building where the apparatus is stored, — as the instruments are necessary for the experimental demonstrations, — there is no room for the lectures under the roof of Dane Hall, which houses the Bursar's Office and Coöperative Stores. The result is that the instruments must be carried through the yard in rain or shine, an effective way to damage our valuable equipment. But the evils connected with the present locality of the psychological laboratory are not only such as result from its narrowness. Its position on Harvard Square, with the con-

tinuous noise and the vibration of the ground, is perfectly prohibitory for large groups of psychological studies and disturbing for every kind of work for which concentration of attention is a fundamental condition. Finally a psychological laboratory, perhaps still more than a physical one, needs in its whole construction a perfect adaptation to its special purpose; the walls, the shape, and the connection of the rooms, everything must be built, as has been done in other universities, for the special end. We have merely the rooms of the old Law School with thin partitions dividing them. In short everything is in a state which was tolerable during the last few years only because it was felt as provisional, but the time when the psychological laboratory must have really adequate quarters cannot be postponed much longer.

The needs of the psychological work can thus be easily demonstrated to every beholder; but while perhaps less offensive on the surface, the outer conditions of the other branches of the Philosophical Department are not therefore less unsatisfactory. The advanced student of logic or ethics does not need a laboratory, but he needs seminary-rooms with a working library where his work may have a local centre, where he can meet his instructors and his fellow students engaged in related researches, where he may leave his books and papers. To-day all this theoretical work has no home at all; the seminaries seek refuge in an empty room of the laboratory at a late evening hour, in a chance lecture-room, or in private homes; there is nowhere continuity, no place to collect or to deposit, no opportunity to meet beyond official hours, no feeling of coherence suggested by surroundings. The most advanced research work of the country is thus done under external conditions which suggest the spirit of a schoolroom, conditions which deprive students and instructors equally of the chance to make our seminaries the fitting forms for their rich content. But if all this is most deeply felt by the advanced students, it is not less true and not less deplorable for the undergraduate courses. There is nowhere fixity of association between the work and the room. The philosophy courses are scattered over the whole yard, wandering each year from one quarter to the other, creeping in wherever a vacant room can be found, not even the instructors knowing where their nearest colleagues are meeting students. The dignity and the unity of the work are equally threatened by such a state of affairs. There remains not even a possibility for the instructor to meet his students before or after the lecture; his room is filled up to the time when he begins and a new class rushes in before he has

answered questions. A business-like restlessness intrudes into the instruction, and yet philosophy above all needs a certain repose and dignity.

Thus what we need is clear. We need a worthy monumental building at a quiet central spot of the Harvard yard, a building which shall contain large and small lecture-rooms, seminary-rooms, a reading-room, and one whose upper story shall be built for a psychological laboratory, so that under one roof all the philosophical work, metaphysical and ethical, psychological and logical, may be combined. Here the elementary and the advanced work, the lecture courses and the researches, the seminaries and the experiments, the private studies in the reading-room and the conferences and meetings of the assistants would go on side by side. Here would be a real school of philosophy where all Harvard men interested in philosophy might find each other and where the students might meet the instructors.

Such a home would give us first, of course, the room and the external opportunities for work on every plane; it would give us also the dignity and the repose, the unity and the comradeship of a philosophical academy. It would give us the inspiration resulting from the mutual assistance of the different parts of philosophy, which in spite of their apparent separation are still to-day parts of one philosophy only. All this would benefit the students of philosophy themselves, but not less good would come to the University as a whole. The specialization of our age has brought it about that in the organization of a university, even philosophy, or rather each of the philosophical branches, has become an isolated study coördinated with others. The average student looks to psychology as to physics or botany; he thinks of ethics as he thinks of economics or history; he hears about logic as coördinated with mathematics, and so on. The University has somewhat lost sight of the unity of all philosophical subjects and has above all forgotten that this united philosophy is more than one science among other sciences, that it is indeed the central science which alone has the power to give inner unity to the whole university work. Every year our universities reward our most advanced young scholars of philology and history, of literature and economics, of physics and chemistry, of mathematics and biology with the degree of Ph.D., that is of Doctor Philosophiae, thus symbolically expressing that all the special sciences are ultimately only branches of philosophy; but the truth of this symbol has faded away from the consciousness of the academic community. All knowledge appears there as a multitude of scattered sciences and the fact

that they all have once been parts of philosophy, till one after the other has been dismissed from the mother arms, has been forgotten. A school of philosophy as a visible unity in the midst of the yard will renew this truth and thus give once more to the overwhelming manifoldness of intellectual efforts of our University a real unity and interconnection; the external connection of administration will be reënforced by the inner unity of logical interdependence.

The time is ripe for a school of philosophy to play this rôle and to fulfil again its old historical mission, to be the unifying principle of human knowledge and life. The second half of the nineteenth century was essentially controlled by realistic energies, by the spirit of analysis, by the triumph of natural science and technique. But a long time before the century came to an end a reaction started throughout the whole intellectual globe; the synthetic energies again came to the foreground, the idealistic interests were emphasized in the most different quarters; the historical and social sciences make to-day the same rapid progress which fifty years ago characterized the natural sciences, and everywhere in the midst of the empirical sciences there is awakening again the interest in philosophy. In the days of anti-philosophical naturalism scientists believed that philosophy had come to an end and that an unphilosophical positivism might be substituted for real philosophy; to-day the mathematicians and physicists, the chemists and biologists, the historians and economists eagerly turn again and again to philosophy, and on the borderland between philosophy and the empirical sciences they seek their most important problems and discussions. The world begins to feel once more that all knowledge is empty if it has no inner unity, and that the inner unity can be brought about only by that science which enquires into the fundamental conceptions and methods of thought with which the special sciences work, into the presuppositions and ultimate axioms with which they begin, into the laws of mental life which lie at the basis of every experience, into the ways of teaching the truth, and above all into the value of human knowledge, its absolute meaning and its relation to all the other human values — those of morality, beauty, and religion. The most advanced thinkers of our time are working to-day in all fields of knowledge to restore such a unity of human life through philosophy. To foster this spirit of the twentieth century in the life of our University there is no more direct way possible than to give a dignified home to the philosophical work. Such a building ought to be a Harvard Union for scholarly life.

The beautiful building which we see in our minds should not be devoted to a single system of philosophy. In its hall we hope to see as greeting for every student the busts of Plato the Idealist and Aristotle the Realist, of Descartes and Spinoza, of Bacon and Hobbes, of Locke and Hume and Berkeley, of Kant and Fichte and Hegel, of Comte and Spencer, of Helmholtz and Darwin. The School of Philosophy will be wide open to all serious thought, as indeed the members of the Department to-day represent the most various opinions and convictions. This ought never to be changed; it is the life-condition of true philosophy. Yet there is one keynote in all our work: a serious, critical, lofty idealism which forms the background of the whole Department and colors our teaching from the elementary introductions to the researches of our candidates for the doctor's degree. All the public utterances which have come from the Department in recent years are filled with this idealism, in spite of the greatest possible variety of special subjects and special modes of treatment. Here belong The Will to Believe and the Talks to Teachers, by William James, the Noble Lectures and the Glory of the Imperfect, by George Herbert Palmer, Poetry and Religion, by George Santayana, The Principles of Psychology, and Psychology and Life, by Hugo Münsterberg, Jesus Christ and the Social Question, by Francis Peabody, Educational Aims and Educational Values, by Paul Hanus, Shaftesbury, by Benjamin Rand, the Conception of God, and The World and the Individual, by Josiah Royce.

We have sought a name which might give symbolic expression to this underlying sentiment of idealism and might thus properly be connected with the whole building. It cannot be that of a technical philosopher. Such a name would indicate a prejudice for a special system of philosophy, while we want above all freedom of thought. It ought to be an American, to remind the young generation that they do not live up to the hopes of the School of Philosophy if they simply learn thoughts imported from other parts of the world, but that they themselves as young Americans ought to help the growth of philosophical thought. It ought to be a Harvard man — a man whose memory deserves that his name be daily on the lips of our students, and whose character and whose writing will remain a fountain of inspiration. Only one man fulfils all these demands perfectly: Ralph Waldo Emerson. It is our wish and hope that the new, dignified, beautiful home of philosophy may soon rise as the moral and intellectual centre of Harvard University and that over its doors we shall see the name: Emerson Hall — School of Philosophy.

III. Emerson as Philosopher

[The following address was delivered at Harvard University, May, 1903, as part of the Emerson Celebration:]

At the hundredth anniversary of Emerson's birthday, Harvard University is to take a noble share in the celebration. For years it has been one of the deepest desires of the Harvard community to erect in the college yard a building devoted to philosophy only. To-day this building is secured. To be sure, the good-will of the community must still do much before the funds allow the erection of a building spacious enough to fulfil our hopes; but whether the hall shall be small or large, we know to-day that it will soon stand under the Harvard elms and that over its door will be inscribed the name: Ralph Waldo Emerson. No worthier memorial could have been selected. Orations may be helpful, but the living word flows away; a statue may be lasting, but it does not awaken new thought. We shall have orations and we shall have a statue, but we shall have now, above all, a memorial which will last longer than a monument and speak louder than an oration: Emerson Hall will be a fountain of inspiration forever. The philosophical work of Harvard has been too long scattered in scores of places; there was no unity, philosophy had no real home. But Emerson Hall will be not only the workshop of the professional students of philosophy, will be not only the background for all that manifold activity in ethics and psychology, in logic and metaphysics, in æsthetics and sociology, it will become a new centre for the whole University, embodying in outer form the mission of philosophy to connect the scattered specialistic knowledge of the sciences. Harvard could not have offered a more glorious gift to Emerson's memorial.

But the spirit of such a memorial hour demands, more than all, sincerity. Can we sincerely say that the choice was wise, when we look at it from the point of view of the philosophical interests? It was beautiful to devote the building to Emerson. Was it wise, yes, was it morally right to devote Emerson's name to the Philosophy Building? Again and again has such a doubt found expression. Your building, we have heard from some of the best, belongs to scientific philosophy; the men who are to teach under its roof are known in the world as serious scholars, who have no sympathy with the vague pseudo philosophy of popular sentimentalists; between the walls of your hall you will have the apparatus of experimental psychology, and you will be expected to do there the most critical and most con-

sistent work in methodology and epistemology. Is it not irony to put over the door, through which daily hundreds of students are to enter, the name of a man who may be a poet and a prophet, a leader in literature and a leader in life, but who certainly was a mystic and not a thinker, an enthusiast but not a philosopher? Not only those who belittle him to-day and who short-sightedly deny even his immense religious influence, but even many of Emerson's warmest admirers hold such an opinion. They love him, they are inspired by the superb beauty of his intuitions, but they cannot respect the content of his ideas, if they do not wish to deny all their modern knowledge and scientific insight. Yes, for the most part they deny that his ideas form at all a connected whole; they are aphorisms, beautiful sparks. Did he not himself say: "With consistency a great soul has simply nothing to do. He may as well concern himself with his shadow on the wall." And yet how can there be philosophy without consistency; how can we interpret reality if we contradict ourselves? If Emerson's views of the world did really not aim at consistency and did really ignore our modern knowledge, then it would be better to go on with our philosophical work in Harvard without shelter and roof than to have a hall whose name symbolizes both the greatest foe of philosophy, the spirit of inconsistency, and the greatest danger for philosophy, the mystic vagueness which ignores real science.

But Emerson stands smiling behind this group of admirers and says, "To be great is to be misunderstood." Yes, he did say, "A foolish consistency is the hobgoblin of little minds, adored by little statesmen and philosophers and divines;" but he soon adds, "Of one will the actions will be harmonious however unlike they seem." Emerson despises the consistency of the surface because he holds to the consistency of the depths, and every sentence he speaks is an action of the one will, and however unlike they seem they are harmonious, and, we can add, they are philosophical; and, what may seem to these anxious friends more daring, they are not only in harmony with each other, they are in deepest harmony with the spirit of modern philosophy, with a creed which ought to be taught by the most critical scholars of Harvard's Philosophy Hall.

What is the essence of Emerson's doctrine in the realm of philosophy? It seems like sacrilege to formulate anything he said in the dry terms of technical philosophy. We must tear from it all the richness and splendor of his style, we must throw off the glory of his metaphor, and we must leave out his practical wisdom and his religious emotion. It seems as if we must lose all we love. It is as if we were

to take a painting of Raphael and abstract not only from the richly colored gowns of the persons in it, but from their flesh and blood, till only the skeletons of the figures remained. All beauty would be gone, and yet we know that Raphael himself drew at first the skeletons of his figures, knowing too well that no pose and no gesture is convincing, and no drapery beautiful if the bones and joints fit not correctly together. And such a skeleton of theoretical ideas appears not only without charm, it appears necessarily also uninteresting, without originality, commonplace. All the philosophies, from Plato to Hegel, brought down to their technical formulas, sound merely like new combinations of trivial elements, and yet they have made the world, have made revolutions and wars, have led to freedom and peace, have been mightier than traditions and customs; and it is true for every one of them that, as Emerson said, "A philosopher must be more than a philosopher."

There are, it seems, three principles of a philosophical character without which Emerson's life-work cannot be conceived. To bring them to the shortest expression we might say, Nature speaks to us; Freedom speaks in us; the Oversoul speaks through us. There is no word in Emerson's twelve volumes which is inconsistent with this threefold conviction, and everything else in his system either follows immediately from this belief or is a non-essential supplement. But that threefold faith is a courageous creed indeed. The first, we said, refers to Nature; he knew Nature in its intimacy, he knew Nature in its glory; "Give me health and a day and I will make the pomp of emperors ridiculous." And this Nature, that is the assertion, is not what natural sciences teach it to be. The Nature of the physicist, the dead world of atoms controlled by the laws of a dead causality, is not really the Nature we live in; the reality of Nature cannot be expressed by the record of its phenomena, but merely by the understanding of its meaning. Natural science leads us away from Nature as it really is. We must try to understand the thoughts of Nature. "Nature stretches out her arms to embrace man; only let his thoughts be of equal greatness;" and again Emerson says, "All the facts of natural history taken by themselves have no value, but are barren like a single sex; but marry it to human history and it is full of life;" and finally, "The philosopher postpones the apparent order of things to the empire of Thought."

And in the midst of Nature, of the living Nature, we breathe in freedom; man is free. Take that away and Emerson is not. Man is free. He does not mean the freedom of the Declaration of Inde-

pendence, a document so anti-Emersonian in its conception of man; and he does not mean the liberty after which, as he says, the slaves are crowing while most men are slaves. No, we are free as responsible agents of our morality. We are free with that freedom which annuls fate; and if there is fate, then freedom is its most necessary part. "Forever wells up the impulse of choosing and acting in the soul." "So far as man thinks he is free." "Before the revelations of the soul, time, space, and nature shrink away." "Events are grown on the same stem with the personality; they are sub-personalities." "We are not built like a ship to be tossed, but like a house to stand." This freedom alone gives meaning to our life with its duties, and puts the accent of the world's history on the individual, on the personality: "All history resolves itself very easily into the biography of a few stout and earnest persons," and "An institution is the lengthened shadow of a man."

Nature speaks to us, Freedom speaks in us, but through us speaks a Soul that is more than individual, an over-individual soul, an "Over-soul, within which every man is contained and made one with all others." Now even "Nature is a great shadow, pointing always to the sun behind her." Every one of us belongs to an absolute consciousness which in us and through us wills its will; "Men descend to meet" and "Jove nods to Jove from behind each of us." Yes, "Man is conscious of a universal soul within or behind his individual life, wherein as in a firmament justice, truth, love, freedom arise and shine." The ideals, the duties, the obligations, are not man's will but the will of an Absolute.

Does not all this sound like a wilful denial of all that has been fixed by the sciences of our time? Does not every Sophomore who has had his courses in Physics, Psychology, and Sociology know better? He knows, we all know, that the processes of Nature stand under physical laws, that the will of man is the necessary outcome of psychological laws, that the ideals of man are the products of human civilization and sociological laws. And if every atom in the universe moves according to the laws which physics and chemistry, astronomy and geology, have discovered, is it not anti-scientific sentimentality to seek a meaning and thoughts in the mechanical motions of the dead world of substance? So the poet may speak, but we ought not to say that his fanciful dreams have value for scholarly philosophers. The philosophy of the scientist ought to be the acknowledgment that matter and energy, and space and time are eternal, and that the smallest grain of sand and the largest solar system move meaningless by blind causality.

And emptier still is the naïve belief that man is free. Do we not profit from decades of psychological labor, whereby the finest structure of the brain has been discovered, wherein the psychological laws have been studied with the exactitude of a natural science, wherein we have studied the mental life of animals and children, and have observed the illusions of freedom in the hypnotized man and in the insane? Yes, we know to-day that every mental act, that every psychological process is the absolutely necessary outcome of the given circumstances; that the functions of the cells in the cortex of the brain determine every decision and volition, and that man's deed is as necessary as the falling of the stone when its support is taken away. Yes, modern psychology does not even allow the will as an experience of its own kind; it has shown with all the means of its subtle analysis that all which we feel as our will is only a special combination of sensations which accompany certain movement-impulses in our body. Can we still take it seriously, when the philosopher steps in and pushes sovereignly aside all the exact knowledge of mankind, and declares simply "Man's will is free!".

Finally, the claim for the over-personal, absolute consciousness in man. It is a triumph of modern science to understand how the duties and ideals have grown up in the history of civilization. What one nation calls moral is perhaps indifferent or immoral for another people or for another time; what the one calls beautiful is ugly for the other; what one period admires as truth is absurdity for another; there is no absolute truth, no absolute beauty, no absolute religion, no absolute morality; and sociology shows how it was necessary that just these ideals and just these obligations should have grown up under a given climate and soil, a given temperament of the race, a given set of economical conditions, a given accumulation of technical achievements. Man has made his Absolute, not the Absolute made man, and whatever hopes and fears make men believe, the scholarly mind cannot doubt that these beliefs and idealizations are merely the products of the feelings and emotions of individuals bound together by equal conditions of life. Leave it to the raptures of the mystic to ignore all scientific truth, to get over-soul connection beyond all experience. In short, to accept Emerson's philosophy, the scientist would say, means to be a poet where Nature is concerned, means to be ignorant where man is concerned, and means to be a mystic where moral and religious, æsthetic and logical ideals are concerned. Can such be the herald of modern philosophy?

But those who are so proud and so quick are not aware that the

times have changed and that their speech is the wisdom of yesterday. In the history of human knowledge the periods alternate. Great waves follow each other, and while one tendency of scientific thought is ebbing, another is rising; and there is no greater alternation than that between positivism and idealism. The positivistic period of natural science has ebbed for ten or fifteen years; an idealistic one is rising. Emerson once said here in Harvard that the Church has periods when it has wooden chalices and golden priests, and others when it has golden chalices and wooden priests. That is true for the churches of human knowledge too, and for knowledge of all denominations. Forty, fifty years ago, in the great period when Helmholtz discovered the conservation of energy and Darwin the origin of species, one naturalistic triumph followed the other, golden high priests of natural science were working with wooden chalices in narrow, awkward laboratories; to-day natural science has golden chalices provided in luxurious institutions, but there are too many wooden priests. The fullest energies of our time are pressing on to an idealistic revival, are bringing about a new idealistic view of the world, and turning in sympathy to that last foregoing period of idealism of which Ralph Waldo Emerson was perhaps the last original exponent. But also with his period idealism was not new. When he came to speak on the Transcendentalist, he began, "The first thing we have to say respecting the new views here in New England is that they are not new." Yes, indeed; since the beginnings of Greek philosophy, more than two thousand years ago, the two great tendencies have constantly followed each other. Each one must have its time of development, must reach its climax, must go over into undue exaggeration, and thus destroy itself to make room for the other, which then begins in its turn to grow, to win, to overdo, and to be defeated.

Glorious had been the triumph of Positivism in the middle of the eighteenth century when the French encyclopædists were at work, those men who wrote the decrees for the French Revolution. But before the last consequences of the Positivism of the eighteenth century were drawn, the idealistic counter-movement had started. Immanuel Kant gave the signal, he fired the shot heard round the world; and Fichte followed, whose ethical Idealism changed the map of Europe, and his spirit went over the Channel to Carlyle, and finally over the ocean to these shores of New England and spoke with the lips of Emerson. It is unimportant whether Emerson studied the great transcendental systems in the original; he knew Kant and Schelling probably at first through Coleridge, and Fichte through

Carlyle. But in the mean time Idealism too had exaggerated its claims, it had gone forward to Hegel, and while Hegelian thought, about 1830, held in an iron grasp the deepest knowledge of his time, his neglect of positive experience demanded reaction, a counter-movement became necessary, and in the midst of the nineteenth century the great idealistic movement with all its philosophical and historical energies went down, and a new Positivism, full of enthusiasm for natural science and technique and full of contempt for philosophy, gained the day. With logical consistency, the spirit of empiricism went from realm to realm. It began with the inorganic world, passed into physics, then forward to chemistry, became more ambitious and conquered the world of organisms, and when biology had said its positivistic say, turned from the outer nature of being to the inner nature. The mind of man was scrutinized with positivistic methods; we came to experimental psychology, and finally, as the highest possible aim of naturalism, to the positivistic treatment of society as a whole, to sociology. But naturalism again has overdone its mission, the world has begun to feel that all the technique and all the naturalistic knowledge makes life not more worth living, that comfort and bigness do not really mean progress, that naturalism cannot give us an ultimate view of the world. And above all, the reaction has come from the midst of the sciences themselves. Twenty years ago scientific work received its fullest applause for the neglect of philosophical demands. Ten years ago the feeling came up that there are after all problems which need philosophy, and to-day philosophers, with good or bad philosophy, are at work everywhere. The physicists, the chemists and the biologists, the astronomers and the mathematicians, the psychologists and the sociologists, the historians and the economists, the linguists and the jurists, all are to-day busily engaged in philosophical enquiries, in enquiries into the conditions of their knowledge, into the presuppositions and methods of their sciences, into their ultimate principles and conceptions; in short, without a word of sudden command, the front has changed its direction. We are moving again towards philosophy, towards Idealism, towards Emerson.

Does all this mean that we are to forget the achievements of natural science, and ignore the results of empirical labor, of labor which has given us an invincible mastery of stubborn nature and an undreamed-of power to calculate all processes of the physical and of the psychical world? No sane man can entertain such a notion. Yes, such ideas would contradict the laws which have controlled the

alternation of Idealism and Positivism through the ages of the past. Whenever Positivism returned, it always showed a new face, and the teaching of the intervening period of Idealism was never lost. The naturalism of the middle of the nineteenth century was not at all identical with the naturalism of the middle of the eighteenth; and so Idealism too, as often as it returned to mankind after periods of neglect and contempt, had every time gained in meaning, had every time found increased responsibilities, had every time to do justice to the new problems which the preceding period of Positivism had raised. If Idealism to-day wants to gain new strength, nothing must be lost of all that the last fifty years have brought us, no step must be taken backward, the careful scientific work of the specialists must be encouraged and strengthened, and yet the totality of this work must be brought under new aspects which allow a higher synthesis; yes, a higher synthesis is the problem of the philosopher of to-day. He does not want to be ignorant of natural science and simply to substitute idealistic demands in the place of solid, substantial facts; and he should feel ashamed of the foul compromise with which half-thinkers are easily satisfied, a compromise which allows science its own way till it comes over the boundaries of human emotions, a compromise which accepts rigid causality but pierces little holes in the causal world, making little exceptions here and there that human freedom may be saved in the midst of a world-machinery; a compromise which accepts the social origin of ideals, but claims a mystic knowledge that just our own private pattern will remain in fashion for eternity. No philosophy can live by compromises. If natural science is to be accepted and Idealism is to hold its own, they must be combined, they must form a synthesis in which the one no longer contradicts the other. Just such synthetic harmonization, and not at all a stubborn ignorance of the other side or a compromise with cheap concessions, was the aim of the period from Kant to Emerson. It is merely the naturalistic period which ignores its idealistic counterpart, which delights in its one-sidedness, which is afraid of harmony because it is suspicious of demands for concessions. It is naturalism only which thinks that mankind can walk on one leg.

If we ask where such harmonization can be found, where the great Idealists of the beginning of the last century have sought it, and where our modern philosophy is seeking it again, well aware that by the progress of science in the mean time the difficulties have been multiplied, the logical responsibilities have become gigantic, we cannot do more here than to point out the direction; we cannot go

the way. And it is clear, of course, too, that such an answer has its individual shape, and that no one can promise to give a bird's-eye view of the marching movement while he is himself marching among his comrades. But the individual differences are non-essential. The one great tendency, the Emersonian spirit, if it is rightly understood, is common to them all. What has modern philosophy all over the world to say about that threefold claim concerning Nature, Freedom, and Oversoul? What has it to say when natural science has fully said its say and had its fair hearing, and has been approved as sound and welcome?

A philosopher might answer, perhaps, as follows: You Positivists have done wonderfully with your microscopes and your telescopes, with your chronoscopes and spectroscopes; you have measured and weighed and analyzed and described, and finally explained the whole world which you perceive, and there is nothing in space and time and causality which can escape your search. But did not all that work of yours involve certain presuppositions which you had accepted and which it was not your business to look on critically, but which, nevertheless, may be open to enquiry? Your first claims granted, all may follow; but how is it with the first claims? You examine all that is in space and time, but what are space and time? You examine the material substances and the contents of consciousness, but what is consciousness, and what is matter? You seek the special applications of causality, but what is causality? Well, you reply, you give the facts just as you find them; but do you do that really? And what do you mean by saying that you find the facts? Let us look, at least for a moment, at the very simplest facts with which your work begins. You say there are physical objects made up of atoms, and you describe them as a physicist; and there are mental ideas in consciousness made up of sensations, and you describe them as a psychologist; and both, you say, you are finding. But what does it mean, that you find the physical object outside there and the mental idea of the object inside in you; is that really a statement of your immediate experience? The physicist speaks of this table here before me, outside of me; and the psychologist speaks of my idea of this table, enclosed in my consciousness. Both may do well to speak so; but will you make me believe that I find that doubleness in my experience? If I see this table and want to use it, I am not aware of one table of wooden stuff and another in me of mental stuff. I am not aware of a two-ness at all, and if the physicist says that this wooden table is made up of molecules and has in itself no color and no continuity,

and that the mental idea in me furnishes all those qualities of color and smoothness, but has no solidity, then they speak of two interesting worlds about which I am anxious to know, but certainly neither of them is the world I live in. If I lean on this table I am not aware of a table in my mind at all. I know the one table only, and this one table has its color and its smoothness.

I know what you will answer. You will say, in your immediate experience there are indeed not two worlds of objects, a physical and a psychical; the real thing to which our interests in life refer is not differentiated into a molecular object outside of us and a sensational object in us, but it is clear that every real thing allows a kind of double aspect; we can consider this table in so far as it is common to all of us, in so far as it is a possible object for every one of us, and in so far as it becomes an object for the individual, and we can then call the objects, in so far as they are common property, physical; and in so far as we take the aspect of individual relations, psychical; and as it must be of the highest importance for our practical purposes to discriminate between those two aspects, we have clearly the right to consider the world from the point of view of both the physicist and the psychologist. It is, of course, an abstraction if we leave out in the one case the one side, in the other case the other side of our objective experience; but we gain by that the possibility of constructing two closed causal systems of which each one must have its special conditions of existence, inasmuch as the one is conceived as related to individuals and the other as independent of individuals.

Very true, we should answer. Something like that saves you completely, justifies fully your claim to separate the physical and the psychical worlds of objects, the world of matter and the world of ideas; but can you deny that you have lost your case, are you not now yourself in the midst of philosophical, methodological discussions, which your physics and psychology themselves cannot settle, yet which must be settled before they can enter into their rights; and above all, do you not yourself see now that your whole physics, for instance, is not at all an account of reality, but merely a certain logical transformation of reality; that you do not find the world of physics at all, just as little as you find the psychical ideas, but that you can merely work over and reshape the reality which you find till you construct out of it your world of matter and your world of consciousness? What you believed you would find you have never found, while your construction of physical things may have been most necessary for your purposes; but do not deny that you have left reality far behind you.

And so it is with all your doings. You tell us proudly, for instance, that you show us the deepest nature of the world by showing us the elements which the object contains, and that you thus bring us at least nearer to the essence of things; and yet if we begin to look into your real achievements, we are disappointed again to find that you are far away from even attempting anything of the kind. You tell us that water is hydrogen and oxygen, and if we say "Prove it," you show us simply that you can transform the water into hydrogen and oxygen, and that you can transform these two elements again into water. Is that really what you promise? We want to know what the thing is, and you show us simply how the one thing can be transformed into another thing; and whenever we turn to your wisdom, it is always the same story. You show us always, and most nicely, how the one goes over into the other, but you never show us what the one or the other really is in itself. For your practical purposes the first may be the most important aspect, but do not make us believe, therefore, that it is the only possible aspect. In short, whether science describes or explains, it never gives us what we find in reality, but makes out of reality a new ideal construction in the service of certain purposes, and never gives us the things as they are, but merely the effects and changes which they produce. Are we still, then, to be deeply impressed with the claim of the naturalist that he alone has the monopoly of knowing reality, while we see now that every step of his leads us away from reality? And have we still to be afraid to raise the voice as philosophers with the claim that reality itself must find its expression, that there must be a science which shall give account of reality as we really find it, of nature before it is made up and repolished for the purposes of the physicist? Only if we have such other account of nature, then only do we speak of that nature in which we live and in which we act, and compared with such an account of the fuller reality, the constructed schematism of the physicist must appear, indeed, as Emerson said, "barren like a single sex." Not the slightest result of natural science is depreciated, not the slightest discovery ignored, if we insist that all these so-called facts have a meaning only under certain artificial conditions which set them apart from the reality of our life; and in this reality lies the interest of the philosopher. We have thus no reason to reproach the scientist so long as the scientist does not fancy that his science gives an account of nature as it really is. Both kinds of work are necessary, and the scientist may well speak, as the squirrel in Emerson's poem:

"Talents differ,
All is well and wisely put;
If I cannot carry forests on my back,
Neither can you crack a nut."

Natural science has to crack our nuts, but philosophy has to carry on its back the flourishing forests of life, in which we wander and breathe. And if Emerson is right, to-day and forever, in claiming that the facts of natural science are not expressions of reality, it is only a small step to see that he was not less right in saying that man is free. Consider man as a particle in the physical universe, consider his actions from the point of view of a causal science, and there is no possibility of escaping materialism and fatalism. We must understand every activity as a necessary outcome of foregoing conditions. Psychology must do so, and physics must do the same. The empirical sciences would be disloyal to their own principles if they allowed the slightest exception. The noblest gesture, the greatest word, the bravest action, must be considered by them under the category of causality. They are necessary effects of all the preceding causes. It may be interesting, it may be fascinating to follow such lines with the enthusiastic energy of scholarly research. But are we really obliged to accept the outcome as an ultimate word concerning the meaning of our freedom? "Forever wells up the impulse of choosing and acting in the soul." Is it really merely an illusion? Has responsibility still its moral value, are we the actors of our actions, are we still good, are we still guilty, when every deed follows as necessary effect? Is not, then, the whole constitution of the world, which has made us, responsible whenever we move our hand for good or for bad?

But we know now where we are standing; we know now that the world of objects, of psychical as well as of physical, is a constructed world, constructed for the purpose of satisfying our demand for causal connection; for that world holds causality because it is the world seen from the point of view of causality; and just as there cannot be anything in that world of physical and psychical objects which is not causally connected, just so it cannot have any meaning at all to ask for causal connection before the world is conceived in the service of this artificial construction. Reality in itself is not causal, and to ask for the causes of the real experience of our inner life has not more meaning than to ask how many pounds is the weight of a virtue, and how many inches is the length of our hopes. But we must go farther. To apply the question of cause and effect to our real will means not only that we apply to the real object a standard which belongs to the

artificial or constructed object, but it means above all that we consider as an object something which in reality is not an object at all. The will which the psychologist describes and must describe, the will which has causes and which is thus not free, is a will conceived as an object found in our mind like an idea, something of which we are aware, something whose happening we perceive, and yet if anything is sure it is the immediate experience that we are aware of our will in a way which is absolutely different from the way in which we perceive objects. We do not perceive our will at all, we will it, we strive it, we fight it; yes, we feel ourselves, only in so far as we are the subjects of will. Our will is our personality, which we do not find but which we are, and which stands opposed and separated by the deepest gulf from the world of objects. Those objects are means and purposes of our will, are ends and aims and instruments; but they come in question for us only as we will them, as we like and dislike them, as we approve and reject them. And if we take this world of objects and reconstruct it into the artificial world of physical and psychical things connected by causality, in this very act of reconstruction we feel ourselves as willing, deciding, approving, aiming personalities, whose wills decide, who think the world as causally connected, whose freedom guarantees the value of our conception of a world not free. There is no knowledge but in our judgments; there is no judgment but in our affirming and denying; there is no affirming and denying but in our will. Our will chooses for its purposes to conceive reality as if it were unfree. What a climax of confusion to think that this conception of an unfree world, the conception of science, can itself now condemn the freedom of the will which has chosen. "Freedom is necessary," said Emerson. We can add, necessity itself is merely a purpose determined by freedom. "Intellect annuls fate," Emerson says. We may add, fate is merely an idea of intellect. Let us be psychologists if we want to analyze, to calculate, to explain the unfree man; but let us be philosophers to understand what it means to be a psychologist. Now the synthesis is reached; the real world is free, but we choose for our purposes to conceive the world as unfree, and thus to construct causal sciences.

And if we understand that in reality man is free and that the psychological aspect of man as unfree is a special way of looking on man for special purposes, then suddenly there opens itself before us the vast field of history, and the historical life, which seemed deprived of all interest by the psychological, iconoclastic mood, suddenly wins again a new importance. We feel instinctively that this

free man of reality, this man who is a responsible actor of his actions, he only is the agent of history; and history is falsified and cheapened when it is brought down to a causal explanation of psychological man instead of real man. History had become an appendix of sociology, and what great historians aimed at in the interpretation of the few "stout and earnest personalities" seemed lost in favor of a construction in which the great man and the genius rank with the fool as mere extreme variations of psychological averages. Now suddenly do we understand that history has to deal with the world of freedom, that it has not to explain, but to interpret, that it has not to connect the facts by linking causes and effects, but by understanding the meaning of purposes, their agreement and disagreement, their growth and liberty. Now we understand why Fichte, why Carlyle, why Emerson believes in heroes and hero-worship, why Idealism has been at all times the fertile ground for writing history and for making history, while Naturalism has made technique, and thought in an anti-historical spirit. Our time begins again to think historically. It can do so because it again begins to emancipate itself from its positivistic disbelief in man's freedom and from its unphilosophic superstition that causal science alone is science, that we know only when we explain.

And when we at last stand man to man in full freedom, no longer as psycho-physical constructions but as free personalities, and when we debate and try to convince each other, will you deny that Jove stands behind each of us and Jove nods to Jove when we meet? Would it even have a meaning for us to go on with our talk, should we try at all to convince each other if you thought and I thought, each one for himself, that our will is only our personal will, that there is no over-individual will, no Oversoul behind us? Can we discuss at all if we do not presuppose that there is really a truth which we are seeking in common, that there are certain judgments which we are bound to will, which we are obliged to affirm, which we will, but not as individuals, and of which we take for granted that every one whom we acknowledge at all as a personality must will them too; and if you come with the flippant air of the sceptic and tell me, "No, there is no truth, all is only as it appears to me, there is no objective truth," do you not contradict yourself, are you not saying that at least this, your own statement, expresses objective truth; that you will this with a faith and belief that this will of yours is an over-individual will which is, as such, a duty, an obligation for every one who thinks? Every escape is futile. And all the over-individuality

that lives in our will towards truth comes to us again in our will towards morality. Do not say sceptically that there is no absolute obligation, that you do not feel bound by an over-individual will in your action, that you will do in every moment what pleases you individually. You cannot even speak this sceptical word without contradicting yourself again, as you demand through the fact of your saying it that we believe that you speak the truth and that you thus feel yourself bound not to lie. If you leave us doubtful whether your word was not a lie, the word itself cannot have any meaning. Do not try to dodge the Oversoul. Men live and fight in its purposes, and men descend to meet. It is as Emerson said, "At first delighted with the triumph of the intellect, we are like hunters on the scent and soldiers who rush to battle; but when the game is run down, when the enemy lies cold in his blood at our feet, we are alarmed at our solitude." Let the sociologists triumphantly reduce the ideals to necessary social products of evolution in the same spirit in which the psychologist eliminates the freedom of the individual; but let us never forget that such a social mechanism is as much an artificial construction necessary for its purposes as is the psycho-physical mechanism of individuality. In that reality with which history deals, in which our freedom lies, there our over-individual will comes from deeper ground than from the soil and the food and the climate. Our logical obligations, our ethical duties, our æsthetic appreciations, our religious revelations, in reality they do not come from without, they come from within; but from within as far as we are souls in the Oversoul. There is no duty in the world but the duty which we will ourselves; no outer force, no training, no custom, no punishment can make us have duties. Duty is our will, it may be the duty to think for the ideal of truth, the duty to feel for the ideal of æsthetics, the duty to act for the idea of morality, the duty to have faith in the ideal of religion; but it is always our own will, and yet not our fanciful, personal, individual will. It is a system of purposes upon whose reality all knowledge of the world, and thus the world as we know it, is dependent forever. The wave of Idealism is rising. The short-sighted superstition of Positivism will not lurk under the roof of a new hall of philosophy. To be a true student of the most scientific, of the most scholarly, of the most insistent philosophy means to respect and to study the sciences, the physical and the psychical sciences, but at the same time to understand that natural science is not the science of reality, that psychology does not touch the freedom of man, that no life has a meaning without the relation to the Oversoul. We cannot

write a whole system and a whole text-book on the front of the new building. It must be enough to write there a symbolic word; happy, forever happy, the university which can write over the door of its temple of philosophy the name: Ralph Waldo Emerson.

IV. The Place of Experimental Psychology

[At the opening of Emerson Hall, December 27, 1905, the American Psychological Association discussed the relation of psychology to philosophy; I opened the discussion with the following remarks:]

From the whole set of problems which cluster about psychology and its relation to neighboring sciences, this hour, in which Emerson Hall is completed, and this room, in which I hope to teach psychology to the end of my life, suggest to me most forcibly to-day the one question: Have I been right in housing psychology under this roof? I might have gone to the avenue yonder and might have begged for a psychological laboratory in the spacious quarters of the Agassiz Museum, to live there in peaceful company with the biologists; or I might have persuaded our benefactors to build for me a new wing of the physical laboratory. But I insisted that the experimental psychologists feel at home only where logic and ethics, metaphysics and epistemology keep house on the next floor.

I certainly do not mean that the psychologist ought to mix the records of his instruments with the demands of his speculations, and that he may seek help from the Absolute when the figures of the chronoscope or the curves of the kymograph are doubtful. Experimental psychology is certainly to-day and will be for all future an independent exact discipline with its own problems and methods. No one can insist more earnestly than I do on the demarcation line between the empirical study of mental phenomena and the logical enquiry into the values of life.

Yet I deny that it is a personal idiosyncrasy of mine to try to combine vivid interest in both. There is no antagonism between them; a man may love both his mother and his bride. I am devoted to philosophy, just as I love my native country; and I am devoted to psychology, just as I love the country in which I do my daily work; I feel sure there is no reason for any friction between them.

Of course, on the surface a psychological laboratory has much more likeness to the workshop of the physicist. But that has to do with externalities only. The psychologist and the physicist alike use subtle instruments, need dark rooms and sound-proof rooms, and

are spun into a web of electric wires. And yet the physicist has never done anything else than to measure his objects, while I feel sure that no psychologist has ever measured a psychical state. Psychical states are not quantities, and every so-called measurement thereof refers merely to their physical accompaniments and conditions. The world of mental phenomena is a world of qualities, in which one is never a multiple of the other, and the deepest tendencies of physics and psychology are thus utterly divergent.

The complicated apparatus is therefore not an essential for the psychologist. Of course, we shall use every corner of our twenty-four laboratory-rooms upstairs, and every instrument in the new cases — and yet much of our most interesting work is done without any paraphernalia. Three of the doctor-dissertations which our psychological laboratory completed last year consisted of original research in which no instruments were involved; they dealt with memory-images, with associations, with æsthetic feeling, and so on. Yes, when, a short time ago, a Western university asked me how much it would cost to introduce a good practical training-course in experimental psychology, I replied that it would cost them the salary of a really good psychologist, and besides, perhaps, one dollar for cardboard, strings, rulers, colored paper, wire, and similar fancy articles at five cents apiece.

On the other hand, I do not know a psychological experiment which does not need a philosophical background to bring its results into sharp relief. Of course, you will say, the psychologist deals with facts, not with theories, and has to analyze and to describe and to explain those facts. Certainly he has to do all that; only he must not forget that the so-called "fact" in psychology is the product of complex transformations of reality. A will, an emotion, a memory-image, a feeling, an act of attention, of judgment, of decision — these are not found in the way in which stones and stars are noticed. Even if I choose perceptions or sensations as material for my psychological study, and still more when I call them *my* perceptions and *my* sensations, I mean something which I have found at the end of a long logical road, not at its starting-point, and that road certainly leads through philosophy. Emerson said wisely, "A philosopher must be more than a philosopher;" we can add: A psychologist must be more than a psychologist. First of all, he must be a philosopher.

What would be the result if our laboratory had moved to the naturalistic headquarters? It would be the beginning of a complete

separation from philosophy. Our graduate students would flock to psychological research work without even being aware that without philosophical training they are mere dilettantes. And soon enough a merely psychological doctorate would be demanded. I do not deny at all that such pure psychologists would find enough to do; I should doubt only whether they know what they are doing. There are too many psychologists already who go their way so undisturbed only because they walk like somnambulists on the edge of the roof; they do not even see the real problem; they do not see the depths to which they may fall.

But does the laboratory itself gain from such divorce? Just the contrary. It is evident that everywhere in the world where the psychological laboratory turns to natural science, the experiments deal mostly with sensation, perception, and reaction; while those laboratories which keep their friendship with epistemology emphasize the higher mental functions, experimenting on attention, memory, association, feeling, emotion, thought, and so on. But is it not clear that only the latter work gives to the psychological laboratory a real right to existence, as the former is almost completely overlapped by the well-established interests of the physiologists? If psychology cannot do anything else than that which physiologists like Helmholtz, Hering, Kries, Mach, Bowditch, and the rest have always done so successfully, then experimental psychology had better give up the trade and leave the study of the mind to the students of the organism.

I have said that we ought not to depend on authorities here. Yet one name, I think, ought to be mentioned gratefully in this hour in which the new psychological laboratory is opened for work. I think of Professor Wundt of Leipzig. The directors of the psychological laboratories in Columbia, and Yale, in Clark and Chicago, in Pennsylvania and Cornell, in Johns Hopkins and Washington, in Leland Stanford and Harvard, and many more are his pupils. Some weeks ago, when I did not foresee our present discussion, I told him of Emerson Hall; and a few days ago I got an answer from which, as my closing word, I may quote in translation. Professor Wundt writes to me: "I am especially glad that you affiliated your new psychological laboratory to philosophy, and that you did not migrate to the naturalists. There seems to be here and there a tendency to such migration, yet I believe that psychology not only now, but for all time, belongs to philosophy: only then can psychology keep its necessary independence." Mr. Chairman, these are the words of the

father of experimental psychology. I hope they indicate the policy to which Harvard University will adhere forever.

V. The Psychological Laboratory in Emerson Hall

A monumental staircase leads from the first — the lecture-room — floor of Emerson Hall to the second, the library floor; at the two ends of its broad corridor smaller staircases lead to the third floor, the laboratory. Its general division of space is seen at a glance from the sketch of the ground plan (opposite page 1). Eighteen rooms of various sizes with outside windows form a circle around the central hall which is well lighted by large skylights; but at each end of the hall itself two large windowless spaces are cut off and each of these is divided into three dark rooms. We have thus twenty-four rooms, besides coat-room, toilet-rooms, etc. A further stair leads to the wide attic which is mainly a store-room for the institution.

In order that the laboratory should be adaptable to the most diverse purposes, the permanent differentiation of the rooms has been kept in narrow limits. It seemed unwise to give from the first every room to a special line of research, as the preponderance of special interests may frequently shift; there are years when perhaps studies in physiological and comparative psychology make the largest demand and others in which studies in æsthetical and educational psychology stand in the foreground. A thorough-going specialization, by which special rooms are reserved for tactual studies and others for chronoscope work or for kymograph researches, allows of course certain conveniences in the fixed arrangement of instruments and a certain elaboration of equipment that is built in, but it very much impairs the flexibility of the whole laboratory, and has thus not seemed advisable for an institution whose catholic attitude welcomes investigations as different as those contained in this volume.

To be sure certain constant requirements have demanded a special fitting up of one room as a workshop, one room for the more delicate instruments, one for the beginning course in experimental work, a lecture-room for the courses in comparative psychology, a photography-room, a battery-room, a sound-proof room, the chief animal rooms, and the dark rooms. We have seven light-proof rooms, finished in black, of which two have outside windows for heliostats; of the others, four can be used for optical research; the longest one contains the photometer. Six other rooms, including the lecture-

room, may be darkened by opaque blinds. One contains a partition with door and a grooved window-frame fitted with screens in which openings of any desired size and shape may be cut. This window is opposite the main door of the room, and opposite this, across the central hall, some sixty feet away, is the door of another dark room; optical stimuli can thus be given from this window to a subject over seventy feet away.

Several rooms are fitted up with special reference to the investigation of the various forms of organic movement, animal behavior and intelligence. As one result of several investigations in animal psychology already pursued here, the laboratory has a considerable number of devices for testing and making statistical studies of the senses and intelligence, methods of learning and emotional reactions of animals.

Adequate provision is made for the keeping of animals in a large, well-lighted, and well-ventilated corner room. Instead of having aquaria built into the room, an aquarium-table eighteen feet long has been constructed to support moveable aquaria of various sizes. Whenever it is desirable for the purposes of an investigation, any of these aquaria may be moved to the research-room of the investigator or to such quarters as the special conditions of the experiment demand.

The vivarium-room contains, in addition to provisions for water-inhabiting animals, cages of a variety of forms and sizes. The largest of these cages, six and a half feet high, six feet wide, and four feet deep, may be used for birds, monkeys, or any of the medium-sized mammals. Cages for rabbits, guinea-pigs, and other small animals are arranged in frames which support four double compartments. Similarly, small cages suitable for mice, rats, and other small rodents are in supporting frames which carry four of the double cages, each of which is removeable and may be carried to the experimenting-room at the convenience of the experimenter.

In a large unheated room above the main laboratory are tanks for amphibians and reptiles. These tanks, since they can be kept at a low temperature during the winter, are very convenient and useful for frogs, tortoises, and similar hibernating animals.

In view of the prime importance of electricity to a modern psychological laboratory, a rather elaborate system of wiring has been designed and built in. The unit of this system is a small delivery-board six inches wide by eight inches high, which carries the following five circuits: *a*, a time-circuit for running magnetic signals; *bb*, two low-tension circuits for chronoscope, bells, signals, etc.; *c*, a high-tension

alternating current (110 v. and 60 phases) for alt. current motors, to be used where great constancy of speed is desired; *d*, a high-tension direct current (110 v.) for dir. current motors, where it is desired to vary the speed continuously (by the introduction of resistance). Two such delivery-boards have been set on opposite walls of all except the smallest rooms, which have but one board. Circuits *a* and *b* are represented on the board by binding-posts, while the high-tension currents, *c* and *d*, appear as flush, protected sockets that take a double-pole plug.

Circuit *a* is a single circuit led from a time-pendulum permanently set in the battery-room, and carried once around the laboratory. It is connected with the *a* binding-posts of the individual delivery-boards in parallel. It follows that the time-circuit is alike for all the rooms at any one time; but in different hours the pendulum can be adjusted to give various impulse-rates. If an investigation requires some special rate of impulse, the special time-apparatus is set up in the investigator's room and current for it taken from one of the *b* pairs of posts.

Each *b* pair goes directly from the delivery-board to the battery-room and ends at a double-pole (telephone type) socket on a large switch-board. Thus every room has two or four direct and independent connections with the battery-room.

The *c* and *d* circuits do not come from the battery-room, but from their respective generators that are stationed outside of the building. They are of course connected at the delivery-boards in parallel.

The large switch-board in the battery-room consists of an upper and a lower part. The upper part bears the double-pole sockets from the *b* posts in all the rooms; the lower part carries some fifty pairs of single-pole sockets that are connected with the batteries stationed near by. These pairs are labelled, and some give a current from cells of the Leclanché type, others of a gravity type. The student has merely to choose the kind and number of cells that he needs, from the lower part, and connect them with one of the double-pole sockets of the upper part which runs to a *b* pair in his own room. By connecting two double-pole sockets with each other, the student can establish a circuit between any two rooms of the laboratory, — this for purposes of telephonic or other communication. Since every room has two, and most of the rooms have four of the *b* circuits, the greatest variety and elasticity of service is attained.

The large switch-board further carries a voltmetre and an am-

metre, both of the Weston make, which are reached (electrically) from double-pole jacks (sockets) on the upper part of the board. Thus before connecting the current with his room, the student can in a moment measure its amount and intensity. These instruments are of the flushface type, and dead-beat.

All of the rooms are lighted by electricity, and the lighting system is independent of the delivery-boards. Nine of the rooms are provided with soapstone sinks, and six (not including the lavatories and service-room) with enamelled iron or porcelain sinks. All the sinks have two taps and each of these ends with a screw-thread so as to take a tip and rubber hose. The soapstone sinks were specially designed with soapstone drip-boards. This is probably the best material for a research-room, and the porcelain and enamel sinks were put only where a neater appearance was desired, or where chemicals were to be frequently used — as for instance in the battery and photographic rooms. Gas is not used for illumination, but six rooms are provided with jets for the smoking of drums, soldering, brazing, etc.

The instrument-room is equipped with large dust-proof cases for holding the more delicate and valuable instruments. The larger unused pieces are stored, out of sight but readily accessible, in an attic which has a clear floor-space of something more than half the total area of the laboratory. Dust-proof cases for demonstration and class-work material are provided in the lecture- and class-rooms.

The shop contains a wood-working bench with two vices, tool-racks, shelves, drawers, cupboards, and stock-racks, for the use of students; and a 9-in. lathe, circular saw, grinding- and buffing-machine, separate bench, vice, racks, and drawers for the use of the mechanic. The machinery is run by a 5 h. p. electric motor suspended from one of the outside brick walls, on brackets. One who selects the equipment of such a shop has to weigh carefully the respective merits of circular and band saws; the latter undoubtedly lends itself to a greater variety of uses, but it is also a far more dangerous machine to have running in a room to which students are to be given access. This latter consideration determined in the present case the choice of a circular saw. It is quite dangerous enough, and may be used only by, or under the supervision of, the mechanic.

It has been stated on competent authority that a truly sound-proof room cannot be built except under ground. This has not been attempted, but the laboratory contains one room (no. 17) which is virtually sound-proof. A double door separates it from

the adjoining experimenter's room, and double doors also separate this from the main hall. The wall between these two rooms consists of two layers of plaster with special deadening material inserted between. Two small tubes, ordinarily stuffed with felt, connect these rooms. When the acoustical stimulus is a tuning-fork, it is placed in a distant room, connected with one of the *b* circuits of the sound-proof room, and then with a telephone receiver near the subject's ear.

The photographic-room contains the ordinary sink, red lights, shelves, etc. The indirect entrance is light-tight when the door is not closed, so that the experimenter may pass in and out even when developing is going on. This room, like all the others which have no window (except the sound-proof room), has forced ventilation.

The class-room is designed for the experimental training-courses. It has eight of the regular delivery-boards, ten tables, instrument-case, blackboard, and sink.

The lecture-room for specialized courses in comparative and experimental psychology seats eighty students. It is provided with two Bausch and Lomb electric projection-lanterns, horizontal and vertical microscope attachments, and attachment for the projection of opaque objects. On the lecturer's platform, besides the blackboard, projection-screen, and chart-racks (capable of holding twenty charts), is a large demonstration-table provided with a delivery-board, water, gas, sixteen chart-drawers, two other drawers, and three cupboards.

As has been said before, the general psychology course of the University is not given on the laboratory floor, but downstairs in the large lecture-hall with about 400 seats. A number of large demonstration instruments of the laboratory serve the special purpose of this course; this hall too has its own stereopticons.

Our instrumentarium is, of course, in first line, the collection of apparatus bought and constructed through the fourteen years of work. Yet with the new expansion of the institute a considerable number of psychological, physical, and physiological well-tested instruments has been added. Especially in the departments of kymographic, chronoscopic, and optical apparatus the equipment presents a satisfactory completeness; its total value may be estimated to represent about twelve thousand dollars. Yet the place of the laboratory which we appreciate most highly is not the instrument-room but the workshop, in which every new experimental idea can

find at once its technical shape and form. Whether those experimental ideas will be original and productive, whether their elaboration will be helpful for the progress of our young science, in short, whether the work in the new laboratory will fulfil the hopes with which we entered it, may be better decided as soon as a few further volumes of the Harvard Psychological Studies shall have followed the present one, which is still from cover to cover a product of Harvard's pre-Emerson-Hall period.

OPTICAL STUDIES

STEREOSCOPIC VISION AND THE DIFFERENCE OF RETINAL IMAGES

BY G. V. HAMILTON

THE question which the Laboratory proposed to me for experimental enquiry was one which demanded a definite reply of yes or no. The positive answer seemed a necessary consequence of the traditional psycho-physiological theories, while a certain practical consideration seemed to suggest the negative solution. The question which seems to have been overlooked so far was this: According to the theory of stereoscopic vision two points of light which are seen by each of the two eyes under the same angle appear to lie in the same plane; as soon as the angle for the right eye is larger than that for the left, that is, as soon as the two stimulated retinal points in the right eye are more distant than the two retinal points stimulated in the left eye, the right light-point seems to be farther away than the left one. If we relate them to planes vertical on the ideal binocular fixation-line, the right point lies in a more distant plane. This principle, which, of course, controls all arrangements for stereoscopic effect, is deduced from experiences in which the fixation-line is vertical to the line that connects the nodal points of the two eyes; the plane in which the equally distant points lie is then parallel to the forehead. If, on the other hand, the eyes are turned to the side, that is, if the ideal fixation-line forms an acute angle with the line connecting the eyeballs, the two fixated light-points, which lie in a plane perpendicular to the fixation-line, cannot be seen by the two eyes under the same angle. Any object on my right side is somewhat nearer to my right eye than to my left, and therefore must throw a larger image on my right retina. The two light-points of a plane vertical to the fixation-line give thus with the eyes turned to the right two unequal pairs of retinal stimuli; and the difference of the retinal stimulations is evidently just the same as if the eyes were looking straight forward but the two lights were at different distances. If difference of retinal images really produces the con-

scious experience of seeing the lights in differently distant planes, vertical to the fixation-line, it follows that with the eyes turned to the right, lights which objectively lie in the same plane must appear subjectively to lie in different distances. The question arises whether the facts correspond to this conclusion. If we look with eyes turned sidewise towards a plane vertical to the direction of seeing, do the points of that plane remain in it for consciousness or do we see them in different planes? We see that practical considerations suggest a "No" to this question, because it would mean that everything which does not lie exactly in front of us must change its plastic form, and this the more strongly the more we see it on our right or our left, and this of course again the more strongly the nearer it is to the eyes, inasmuch as the relative difference of the retinal images must increase with the nearness of the object. If a short-sighted person fixates an object a few centimetres from the eyes strongly turned to the side, the distances in the retinal image of the one eye may be almost the double of those in the other. Under normal conditions the differences would be smaller, but yet everything would be necessarily distorted in its three-dimension shape as soon as it is seen in indirect vision or with sidewise fixation. On the other hand, if the objects keep their three-dimensional relations in spite of sidewise movements, it is evident that the accepted psycho-physiological theory of stereoscopic vision is incomplete and must be revised in a very essential way. The experiment had to decide. Of course the question might be approached experimentally in different ways. It would have been possible, for instance, to study the stereoscopic synthesis of two separate flat pictures seen with the eyeballs in different positions. But we preferred the simplest possible way, seeking the threshold of distance for two parallel vertical edges with eyes turned forward and to the side. We chose edges instead of hanging threads for the purpose of avoiding the possible influence of the apparent thickness of the threads on the judgment of distance. Of course, distance is here never distance from the one or the other eye, but from the centre of the line which connects the two nodal points of the eyes; the two vertical planes whose edges were to be compared stood always vertical on the ideal line of fixation which starts from that central point between the two eyeballs.

The apparatus used in these experiments consists of three parts, viz.:

(1) A plank 2.5 metres x 9.5 centimetres x 4 centimetres, set on edge and screwed to a table at either end.

(2) A head-rest 45 centimetres high, 35 centimetres broad and 15 centimetres deep. Attached to the centre of the lower strip of the frame is a concave trough for the chin. Another trough, shaped to fit over the vertex and with a strip of wood fastened to the front of it for the forehead, slides up or down within the frame. The attachment for the forehead can be moved and fixed at various positions antero-posteriorly. By means of these devices the head can be securely fixed in any position desired without discomfort to the subject.

In order to have the eyes always in the same plane and at a known distance from the apparatus at the other end of the plank, a hole was made in either side of the frame with its centre at a level of the eyes. Extending through the vertical diameter of each hole is a fine wire. Fitted into the inner portion of each hole is a cardboard tube 10 centimetres long: the inner end of each tube contains a vertical wire so arranged that the four wires all fall into a plane at right angles to the long direction of the plank. A mirror at the outer exit of either hole enables the experimenter to align the tips of the subject's corneæ with the wires.

Two parallel strips of wood are so attached to the "head-rest" end of the plank — one below and the other above it — that they can be rotated laterally upon the plank, with the bolt which secures them to it for a centre of rotation. Opposite this centre, and attached to the anterior surface of the upper parallel strip is a wire needle 25 centimetres long. By means of a quadricircular piece of cardboard attached to the plank at the end of the needle, the extent of rotation to the right or left can be read off in degrees. (The point midway between the two corneal tips when they are aligned with the wires is in the same axis of rotation as the head-rest.)

A vertical iron rod 50 centimetres long extends upwards from either end of the parallel strips, and upon these rods the frame of the head-rest can be moved up or down by means of thumb-screws upon which it rests.

(3) At the opposite end of the plank there is attached a flat board, 35 centimetres long and 30 centimetres wide. Attached to the edge of the board which faces the head-rest is a piece of black cardboard 40 centimetres long by 35 centimetres broad. In the centre of the cardboard is a rectangular aperture, 7 centimetres by 14 centimetres. On the upper surface of the board are two slots, one at either side. Sliding within each of these slots is a block of wood to which is attached an upright sheet of black-painted tin, 15 centimetres wide and 20 centimetres high. The surfaces of these tins lie in planes parallel to

the plane of the four wires in the head-rest, when the latter is at right angles to the plank. When their surfaces are equidistant from the wires, the inner vertical edges of the tins are separated from each other by 3 centimetres. The sides of the slots, in which the blocks with their tins slide, are fitted with millimetre scales, thus enabling the experimenter to determine the distance of the edges from the corneæ. The point on the scale at which an edge was exactly 2 metres from the vertical plane of the wires was chosen as the "zero" point, and if this distance was decreased by moving an edge forward, the latter was said to stand at "minus" one, two, or more millimetres, as the case might be. Likewise, an edge was said to stand at "plus" the number of millimetres' distance beyond the zero point if it had been moved at a greater distance than 2 metres from the wires. A piece of ground glass attached to the distal end of apparatus enabled the experimenter to secure a uniform illumination, the room being darkened and the light coming from a 32-candle-power electric lamp set about a metre and a half behind and on a slightly lower level than the glass.

It was found that by shading the lamp itself and admitting a dim light to the room by means of drawing down only the ordinary thin window-shades, the edges could be made to seem almost isolated in space and to stand out in clear relief.

The subjects of the experiment were Messrs. Bell, Flexner, and Tait. Each subject determined the equality-point and the threshold for the normal primary position of the eyes, for the eyes in a lateral position of 15° and in a lateral position of 30°, both to the left and to the right.

Eyes at 0° means the following: that the most anterior part of the two corneæ lies in a plane parallel to and two metres' distance from the plane in which the two parallel edges lie at 0. Eyes at 30° to the left means that a line drawn in front of the two corneæ intersects such a line at an angle of 30°, the left eye being at the distal end of the line. In calculating the visual angles 7.4 mm. are added in order to compensate for the distance from the extreme anterior portion of the cornea to the nodal point of the eye.

The results for Mr. Tait are as follows:

The position of eyes 0°. The right edge was moved, at first from an evident + position to equality, then from equality to the − threshold, then from an evident − position to equality, then from equality to the + threshold. These four points were determined each fifteen times and the average taken. Then exactly the same fifteen sets of

four determinations with the left edge moved. The averages of these 120 experiments are these: When the left edge is moved from + to =: −2.77, from = to −: −6.97, from − to =: +0.77, from = to +5.93. When the right edge is moved from + to =: +2.83, from = to −: −1.6, from − to =: +5.9, from = to +: +10.53. The first equality-point appears thus when the left edge is moved at −0.76, when the right edge is moved at +4.41, with a threshold of about 5 in either case. With the normal eye-position the edges must thus not be exactly in the same plane to appear equally distant; at a distance of 2000 mm. the left must be about 2 mm. nearer than the right to appear in the same plane, vertical to the line of regard.

If the position of the eyes is 15° to the left, we have the following results: When the left edge is moved from + to =: −4.17, from = to −: −8.5, from − to =: −1.33, from = to +: +1; when the right edge is moved from + to =: +4.17, from = to −: +1.17, from − to =: +4.5, from = to +: +8.67.

If the position of the eyes is 30° to the left, we find when the left edge is moved from + to =: −2.67, from = to −: −6.67, from − to =: +0.5, from = to +: +3.33. When the right edge is moved from + to =: +2.33, from = to −: −0.02, from − to =: +9., from = to +: +12.33.

If we take again the general averages, we have for the eye-position of 15° to the left an equality-point of −3.25 if the left edge is moved and judged and +4.63 if the right edge is moved and judged. That is, if the right edge stands at 2000 mm. the left edge must be moved to 1996.75, and if the left stands at 2000, the right must be moved to 2004.63. For the eye-position of 30° to the left, the equality-point lies at −1.49 if the left edge is moved and judged, and at +5.91 if the right edge is the variable. The threshold lies in all three cases, for eyes at 0, at 15°, and at 30°, at about ±5 mm.; the position of the eyes has thus no influence on the threshold for the perception of distance in the direction of regard.

But the point essential for our investigation is of course not the threshold but the equality-point. To take the extremes of the eye-positions 0° and 30° we find the equality when the left edge is judged, at −0.76 for 0° and −1.49 for 30°, and when the right edge is moved, at +4.41 at 0° and +5.91 at 30°; the middle is thus +1.82 for 0° and +2.21 for 30°, that is a difference of less than 0.4 mm.

To understand this figure we must enter into the calculation of the angles. We have an eye-distance of 60 mm., a distance of the edges from the cornea 2000 mm., from the nodal points 2007.4 mm.,

the distance of each edge from the median line 15 mm., the distance of the two edges from each other thus 30 mm. as long as they are in the same plane. We have to determine the angle under which each eye sees the distance of the two edges. A simple trigonometric calculation gives the following figures: If both eyes are in normal position, at 0°, and both edges are in the same plane, 2000 mm. from the corneæ, the angle for each eye is 51′ 22″. If the left edge is now moved to +5, the left eye sees the distance of the edges at an angle of 51′ 25″, the right eye under 51′ 10″, the difference is thus 15″; if the left edge is at +10 mm., the left eye's angle is 51′ 29″, the right eye's angle 50′ 59″, the difference 30″. If the left edge is moved to −5 mm., the left eye's angle is 51′ 18″, the right eye's angle 51′ 33″, the difference 15″; if the left edge is moved to −10 mm., the left eye's angle is 51′ 14″, the right eye's angle 51′ 45″, the difference 31″. Now we saw that with normal eye-position when the left edge was moved the threshold was +5.93 and −6.97; a difference of 15″ to 20″ between the visual angles of the two eyes was thus amply sufficient to give a distinct experience of different distance. When the left eye's angle was about 15″ smaller than the angle of the right eye, the difference of the retinal images gave a sure impression of the greater nearness of the left edge.

If we now bring the eyes into the position of 30°, the angles are of course different when both edges are in the same plane vertical to the direction of regard. If the two edges are in the same plane, the left eye's angle is 50′ 59″ and the right eye's angle 51′ 45″, the difference thus 46″. If we move the left edge to +5, the left angle becomes 51′ 1″, the right angle 51′ 34″, the difference 33″. If we move the left to +10, the left angle becomes 51′ 4″, the right 51′ 24″; the difference is thus still 20″, and we must move the left edge to +17 mm. to get an equal angle for the left and the right eye. If we move the left to −5, the difference becomes of course larger, the left eye sees under 50′ 56″, the right eye 51′ 55″, the difference 59″; and at −10, the left eye has the angle 50′ 53″, the right eye 52′ 6″, difference 1′ 13″. It is hardly necessary to state here the angles for the changes of the right edge or for an eye-position of 15°, inasmuch as the maximum differences bring out our case most clearly. With an eye-position of 15°, the edges at the same plane give angles of 51′ 10″ and 51′ 34″, that is, a difference of 24″; if the left edge is moved to −5 mm. the difference becomes 38″; if it is moved to −10 mm. the difference is 54″; if the left edge is moved to +5 the difference decreases to 10″ and at +10 mm. to 6″.

We have thus the following fundamental result: If the eyes are in normal primary position, a movement of the left edge to ±6 mm. is constantly apperceived at threshold of distance and this corresponds to retinal images whose visual angles differ by about 17″. A difference of 17″ in the visual angles of the two eyes produces thus under the conditions of this experiment for this subject a strong stereoscopic effect when the eyes are in primary position. If the eyes are in the position of the head 30° to the left, the left eye thus much further from the edges than the right eye, the visual angle of the left image thus much smaller than that of the right image, we find the same equality-point with the same threshold. We saw that in this position the two visual angles would be equal if the left edge were moved to +17 mm.; instead of at +17, the equality-point — when the left edge is judged — lies at −1.49, that is, at a point at which the visual angle of the left eye is more than 46″ smaller than the angle of the right eye. While in normal position a difference of the two retinal images of 17″ constitutes a distinct threshold value; at a lateral position of the eyes of 30° the great difference of 46″ becomes necessary to give the impression of equal plane, while a decrease of that difference to 30″ gives a distinct feeling of greater distance. Equal retinal images produce for the lateral eyes thus the same effect which for the normal position very different images produce; and to get for the lateral eyes the effect which equal images produce for the normal position, the angles of the images must differ by 46″.

The results for the second subject, Mr. Flexner, are practically the same. With the position of the eyes at 0°, when the left edge is judged and moved, we find the following averages: from + to = : +0.03, from = to − : −3.8, from − to = : −0.7, from = to + : +3.93; when the right edge is moved from + to = : −0.08, from = to − : − 4.29, from − to = : +1.21, from = to + : +4.08. It is evident that the difference between right and left which existed for Mr. Tait does not enter into Mr. Flexner's results. The equality-point as average of 120 experiments lies for normal eye-position practically at zero, and the threshold is ±4 mm.; his sensibility for differences of retinal images is thus still finer than for Mr. Tait, as we saw that the threshold of ±4 mm. means a difference of visual angles of less than 15″. If Mr. Flexner's head is turned 15° to the left, his left eye thus considerably farther away from the edges than the right eye, the results are these: If the left edge is moved and judged, we find from + to = : −0.02, from = to − : −3.17, from − to = : 0, from = to + : +4.67; if the right edge is moved from + to = : −0.01, from = to − : −2.5,

from − to = : −0.8, from = to + : +3.33. Experiments with lateral movement of 30° were not carried through, as the subject, accustomed to eye-glasses, became less accurate in the judgments; but the experiments with the position of 0° and of 15° are unequivocal. They show that the equality-point and the thresholds are exactly the same for 15′ as for 0°. For the lateral position of 15° again the average equality-point is exactly at 0° and the threshold at less than ±4 mm. We saw that for a lateral movement of 15° the difference of the angles at the equality-point is 24″. We find thus for Mr. Flexner that with primary eye-position a difference of angles of less than 15″ gives a distinct stereoscopic effect, while with a lateral position of the eyes a plane effect demands a difference of 24″ for the two visual angles.

Experiments with Dr. Bell finally showed a rather strong fluctuation of judgments and the determination of the equality-point for normal eye-position has not only too large a middle variation to be a reliable basis, but is influenced by a constant tendency to underestimate the distance of the edge moved. Yet the general result is the same as with the other two subjects, that is, the equality-point is with him, too, practically the same for the eyes in normal and in lateral position.

The general conclusion from the results of all three subjects is thus evidently that the traditional physiological theory is untenable, the stereoscopic effect cannot be simply a function of the difference of the two retinal images. The same pair of unequal retinal images which gives a most striking stereoscopic effect for eyes in primary position, has no stereoscopic effect for eyes in lateral position and *vice versa.* The stereoscopic interpretation is thus the function of both the difference of the retinal images and the position of the eyeballs. Of course the two retinal images are in any case never felt as two pictures if they are not different enough to produce a double image. With the primary position of the eyes as long as the two different retinal views are sufficiently similar to allow a synthesis in a three-dimensional impression of our object, we perceive every point of the object not as double image but as one point of a given distance. The distance feeling of the normal stereoscopic vision demands thus itself more than the reference to the different retinal images, and the only factor which can explain the phenomena is the response of the eye-muscles which react on the double images by increase or decrease of convergence. The distance of a point in a stereoscopic image is determined by the impulse necessary for that particular act of convergence of the eyeballs by which the two retinal images on non-cor-

responding points would be changed into images on corresponding points. The different retinal images are thus ever for the normal eye-position merely the stimuli for the production of that process which really determines the experience of distance, that is, the motor impulse to a change in convergence.

If thus the stereoscopic vision under normal conditions is ultimately dependent upon the central motor impulses, it is not surprising that a change in the psycho-physical conditions of movement produces a change in the resulting impulses. Such a change in the conditions is given indeed whenever the eyes are in a lateral position. Just as the same stimulus produces a different response when the arm or leg is in a flexed or an extended position, so the retinal double images stimulate different responses according to the particular position of the eyeballs. That pair of unequal retinal images that in primary eye-position produce in going from one end of the object to the other a strong increase of convergence and thus a feeling of greater nearness, may produce with the lateral eye-position no increase of convergence and thus a feeling of equal distance or even a decrease of convergence and thus a feeling of removal. The psycho-physical system upon which our three-dimensional visual perception depends is then much more complex than the usual theory teaches; it is not the retinal image of the double eye, but this image together with the whole distribution of contractions in the eye-muscles, which determines the stereoscopic vision: the same retinal images may give very different plastic perceptions for different positions of the eyeballs.

The experiments point thus to the same complex connection which Professor Münsterberg emphasized in his studies of the "Perception of distance." [1] I may quote the closing part of his article to bring out the intimate connection of the two problems. He reports his observations on the so-called verant and insists that the monocular verant almost as little as the ordinary binocular stereoscope can give the impression of normal distance of nature. Professor Münsterberg writes: "Whoever is able to separate seeing in three dimensions from seeing in natural distance cannot doubt that in both cases alike we reach the first end, the plastic interpretation, but are just as far removed from the other, the feeling of natural distance, as in the ordinary vision of pictures. The new instrument is thus in no way a real 'verant.'

"The question arises, Why is that so? If I bring my landscape pic-

[1] Münsterberg: Perception of Distance, The Journal of Philosophy, Psychology and Scientific Methods, vol. 1, p. 617, 1904.

ture on a transparent glass plate into such a distance from my one eye that every point of this transparent photograph covers for my resting eye exactly the corresponding point of the real landscape and yet accommodation is excluded, as, for instance, in the case of the short-sighted eye, or in the case of the normal eye with the verant lenses, then we have exactly the retinal images of the real view of nature and the same repose of the lens. Why are we, nevertheless, absolutely unable to substitute the near object for the far one? This problem exists in spite of all the theoretical assurances that the one ought to appear exactly like the other, and I think that it is not impossible to furnish an answer to it.

"If I am not mistaken, there is one point of difference between seeing the mere picture and seeing the far landscape, which has been neglected in the usual discussions. Every one knows, of course, that we see the picture and the landscape normally with the help of eye-movements. The eye moves from point to point; but psychologists have neglected the consideration that the relation between eye-movement and retinal image must be quite a different one for the landscape and for its photograph. Let us consider the simplest possible case, the case of the myopic eye without any lenses whatever, and without any need of accommodation for a picture as near to the eye as 10 cm. If I take a small landscape picture made with a camera whose distance from lens to plate is 10 cm., I have a splendid plastic view if I see it at a distance of about 10 cm. from my eye. I have before me just such a picture in which two mountain peaks are, in the photograph, 1 cm. distant from each other. If I now have my little picture at the distance of 10 cm. from the eye, these two mountain tops correspond in their distance of 1 cm. exactly to the retinal image which the two real mountains, which are ten miles away and one mile distant from each other, produce in my retina. The retinal image of the two mountain peaks in the photograph is thus for my resting eye indeed identical with that of real nature. Does that mean that I have to make the same eye-movement to go from the left to the right mountain in the landscape as in the picture? Of course, that would be so, the movement would be just as identical as the retinal images if the nodal point of the light-rays were identical with the rotation-point of the eyeball. But everybody knows that this is not at all the case. The light-rays cross in the lens. The angle of vision, and thus the size of the retinal image, are thus dependent upon the distance of the lens from the retina. But the movement of the eye is related to a rotation-point which lies about 13 mm. behind

the cornea, roughly speaking 1 cm. behind the nodal point of the rays. This additional centimetre plays, of course, no rôle whatever, if I look at my mountains in the real landscape; following with my eyeball from the fixation-point of the left mountain to the fixation-point of the right mountain, I make a movement whose angle can be declared identical with the angle under which I saw the two mountains with the resting eye in the first position. This angle of vision was determined by the distance of the nodal point, which was in our case ten miles, while the angle of eye-movement was determined by the distance of the rotation-point, which would be ten miles plus one centimetre, and there is of course no possible difference for practical discrimination between these two distances.

"But the situation is completely changed if I turn to my little picture 10 cm. distant from my eye. The angle under which I see my two peaks is, of course, again the same under which I saw them in the real landscape. It is determined by the distance of the picture from the nodal point, which is in this case 10 cm. But the angle of the eye-movement necessary to fixate first the left and then the right peak is now a much smaller one because it is again determined by the distance from the rotation-point, and that is in this case 10 cm. plus 1 cm. With this short distance of the picture from the eye this one additional centimetre is not at all the negligible quantity which it was in addition to ten miles in the landscape. For the two real mountains the angle of the eye-movement had a tangent of one tenth; for the photograph mountains, in spite of their equal size of retinal image, the angle of necessary movement would of course have a tangent of one eleventh. Roughly speaking, we could say that the photograph, in order to produce the same eye-movement which the mountains in the landscape excited, would need a pictorial distance between the two photograph mountains of 11 mm. instead of 10 mm. Of course if the distance in the picture were made 11 mm. instead of 10, it would not cover any more the mountains of the landscape. The retinal image would thus be relatively too large and would not give us any longer the true landscape. On the other hand, if we tried to correct it by bringing the picture one centimetre nearer to the eye, then of course every retinal image would be enlarged by that necessary tenth, and yet there would be no help for the situation, as now again the eye-movement demanded by the retinal image would be relatively increased too.

"We can put it in this way: *my real landscape demands a relation between retinal image and movement which my picture cannot pro-*

duce under any circumstances whatever. That which would be needed to imitate the relations would be realized only if I had my retinal images from the picture at a distance of 10 cm., and at the same time the movements belonging to the same picture seen at a distance of 9 cm. That is of course unrealizable. We cannot see a picture without having our movements constantly controlled by the size of the real retinal images, as it is necessary that the distance seen in indirect vision is the distance covered by the fixation-point during the eye-movement. That demands, as we have seen, a different relation between retinal image and eye-movement for near and far, and no verant and no stereoscope can eliminate this factor. If a 10-mm. object in the photograph demands an 11-mm. movement to give the impression of real natural distance, then we have a condition which cannot be fulfilled.

"If we remember how extremely delicate is our normal sensitiveness for retinal distances and how the newer studies in stereoscopic vision have demonstrated an unsuspected delicacy of adjustment between retinal images and motor responses, it is evident that this so far always neglected relation must be an extremely important one. If we have one adjustment of central reaction in which a certain eye-movement corresponds to retinal images of one size, and another adjustment in which the same movements correspond to retinal images which are ten per cent larger, we can really not expect our judgment of distance to neglect the difference between these two systems of relations. Of course they represent two extreme cases. Every distance beyond 10 cm. demands its special adjustment up to the point where the distance becomes too large to be influenced by the distance from the nodal point to the rotation-point. We must thus presuppose a sliding scale of ever new adjustments for the different distances at which we see any object, and we have, in this relation, probably not the least important factor in the judgment of the third dimension for relatively near objects, and probably even more important than the irradiation circles which control the accommodation, as these circles must be the same for objects which lie before and behind the fixation-point. Of course the whole system of our localizing reactions becomes through these considerations more complex by far than the schematizations of the text-books propose. But physiological optics has shown at every point in its development that mere simplification has not always meant a deeper insight into the real relations."

It is evident that our studies in stereoscopic vision with lateral eye-position involve exactly the same principle and reaffirm completely Professor Münsterberg's theoretical views. In both cases, in the monocular of the verant as in the binocular of our experiments, the same retinal image has different psycho-physiological space-value on account of the different motor situation.

EYE-MOVEMENTS DURING DIZZINESS

BY E. B. HOLT

It is a familiar fact that when the head is passively turned about its vertical axis, the eyes do not move with the head but lag behind, keeping their fixation on that object toward which they were directed before the head moved. The eyes move in their sockets in a direction opposite to that in which the head has moved. Now it has been proved beyond a doubt by the experiments of Mach,[1] Crum Brown,[2] and Breuer,[3] that these lagging movements of the eyes are reflex and are governed by the semi-circular canals, which are stimulated directly by the motion of the head. Similar reflex eye-movements are found when the head is turned about some other than its vertical axis, the direction of such movements being always in confirmation of the theory. All these movements, together with the theory, are well described in the summaries of Peters [4] and Nagel.[5] The present paper deals solely with the eye-movements that occur after rotation of the head about its vertical axis.

The mechanism of these lagging, reflex movements is not, then, identical with that which enables us, when the head is at rest, to fix on and follow a luminous moving object,—the "pursuit movements" of Dodge [6] It is, however, identical with that of Dodge's "fourth type" [7] and that of the compensatory eye-movements described by Brown,[8] Nagel,[9] and Delage,[10] and recently studied by Angier.[11] This

[1] E. Mach: Sitzungsb. d. k. Akad. d. Wissensch., Wien, 1874.
[2] A. Crum Brown: Proceedings of the Royal Soc., Edinburgh, 1874.
[3] J. Breuer: Med. Jahrb., Wien, 1874–75.
[4] W. Peters: Arch. f. d. ges. Psych., vol. 5, p. 42, 1905.
[5] W. Nagel: Handbuch d. Physiol. des Menschen, vol. 3, p. 762, 19[illegible]
[6] R. Dodge: Amer. Jour. of Physiol., vol. 8, p. 317, 1903.
[7] *Ibid.* p. 327.
[8] A. Crum Brown: Proceedings of the Royal Soc., Edinburgh, 1895.
[9] W. Nagel: Zeitsch. f. Psych. u. Physiol., vol. 12, p. 331, 189[illegible]
[10] Yves Delage: Arch. de Zoöl. expér. et générale, vol. 1, 190[illegible]
[11] R. P. Angier: Zeitsch. f. Psych. u. Physiol. vol. 37, p. 235, [illegible]

function of the semi-circular canals was first suggested by Goltz in 1870. Now if the rotary movement of the head is prolonged, the eyes lag for a while on their first fixation-point, and then dart suddenly forward to a new fixation-point on which they rest for a while as before, until they dart forward again. Therefore if the head continues to rotate, the eyes fall into a regular and well-marked nystagmus. In this the lagging movements, or those opposite to the direction of the head, are called "compensatory," and are relatively slow and long. Their rate coincides closely if not exactly with that of the head-movement. But the movements forward, in the direction of the head-movement, are short and swift. Such are the facts during the rotation of the head.

But if this rotation has been somewhat prolonged, the ocular nystagmus continues after the head and body are brought to rest. But now its phases are reversed, and the slower eye-movements are in that direction in which the head has moved; while the swifter are in what before was the lagging direction. These observations are in accord with the semicircular canal theory, and are well established by various investigators.[1]

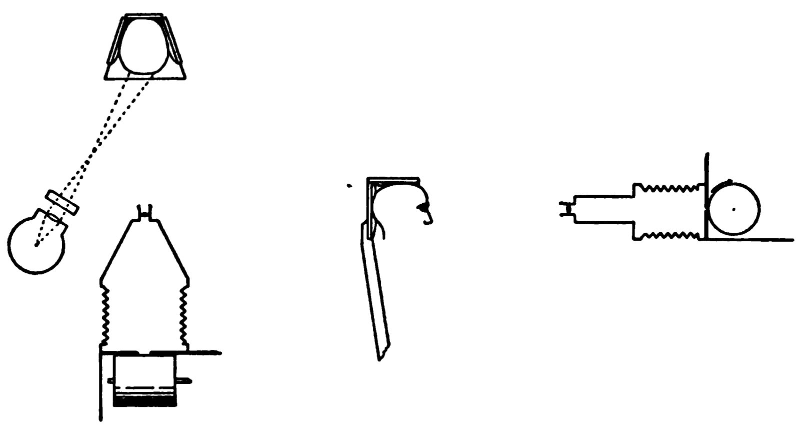
This paper presents the results of a photographic study of the reflex eye-movements following after rotation of the head (and body) about the vertical axis.

The subject whose eyes were to be photographed sat in a chair placed on a rotating platform, in such a position that the vertical axis of rotation passed through, or just posterior to the nose. Rays from an arc-lamp of 6 amperes, placed about 60 cm. from the subject's face, were so converged by a lens that when the subject came to rest, after the rotation, his two eyes were brightly illuminated. An adiathermal screen consisting of a dilute solution of copper ammonium sulphate kept the heat from being painfully intense on the eyes. The light fell slightly from one side on the subject's face, when he was brought to rest; and directly in front of him, at a distance of about 40 cm., was a camera of which the lens was on a level with his eyes. The ordinary ground-glass screen at the back of this camera was replaced by a light-proof box, in the front of which, and in the plane which should have been that of the ground glass, was a slit 55 mm. broad and 5 mm. high. Inside the box was a Ludwig kymograph of which the drum rotated on a horizontal axis: the circumference of the drum lay tangentially to the front of the box, and the line of tangency passed horizontally through the long axis of the slit. For

[1] See the summaries of Nagel and Peters, above referred to.

each photograph a photographic film of sensitometer 40 was fixed to the drum, as paper is ordinarily fastened, and in moving, the drum carried this film upwards past the slit. It follows from this arrangement that 5 mm. along the length of this film were always exposed at once. The camera was so focused that the images of both eyes were sent through the slit, and fell on the film.

The subject's head was rigidly held by a rest: this rest was adjusted, and the camera focused, before the rotation. The adjustment of the head was greatly facilitated by fastening a fine black thread to pegs that projected forward from the head-rest, on either side; the thread was stretched horizontally, and at such a height that



Figs. 1 and 2

its image in the camera coincided with the middle of the long (horizontal) axis of the open slit. If then the subject, on seating himself in the chair, had his head so adjusted that each eye was directly behind the thread, each eye would certainly be imaged on the sensitive film. Neither the shadow of this thread on the subject's face, nor its image on the film, interfered in the least with the exposure that was made after rotation. This thread was further found very useful by the subject himself, who, after the rotation and just before the exposure was made, could make sure by sighting on the thread that his eyes had not slightly changed position during the rather protracted rotation. The subject was ordinarily turned twenty-five times at about the rate of one turn in two seconds. The kymograph was set in motion and the exposure commenced as soon as the whirling chair was brought to a dead stop. This stopping always took two o

three seconds, at the very time when the nystagmus was most pronounced, so that the photographs do not show the maximum eye-movements. The exposure lasted through one rotation of the drum, nine seconds.

In the strongest negatives the movements of the eyes can be fairly well made out from the undulatory curve generated on the film by the dark image of the iris as it oscillated from side to side. But this is true only of the best negatives, and almost never of these if the eyes photographed had the iris blue. In order to obtain better definition in the photographs of the eye-movements, small flecks of Chinese white were tried, as invented and described by Judd.[1] A small square of white was laid with a brush on each cornea, on the side toward the lamp, so that its image on the film should be as bright as possible. The flecks were found to adhere to the eyeball even more perfectly than Judd himself has claimed; and they produced so little discomfort that the subject ordinarily forgot their presence on the eyes. Nevertheless their image as produced on the negatives, although much better than that of the iris, was generally not clearly readable, owing to the brief exposure and the illumination by electric light. This light seems not to be well reflected by the Chinese white: but in all cases where daylight can be employed the use of these flecks must be eminently satisfactory.

Thus it was found necessary to fall back on the image of the arc as reflected from the cornea. This corneal image invariably traced a clear, strong curve on the negative, and would have been appropriated at the outset, were it not that its movements are not, as is well known, a true register of the *amplitude* of the corresponding eye-movements; a fact that was shown clearly from a comparison in these negatives of the curves produced respectively by the flecks of Chinese white and by the corneal image. The former showed a much greater amplitude of movement. But the corneal reflection is a perfect register of the *time* and *direction* of the eye-movements; and in the following tables these features alone are studied. This reflection traced on the film a perfectly readable curve, although in some of the films, owing to a shifting of the carbons in the lamp taking place during the rotation, one of the eyes would be badly illuminated and a good record would be obtained from the other eye alone.

The arc ran on an alternating circuit of 60 phases per second, and owing to these interruptions of the illumination the curve of the

[1] C. H. Judd: Yale Psych. Studies, Psych. Rev., Mon. Supplements, vol. 7, no. 1, p. 7, 1905.

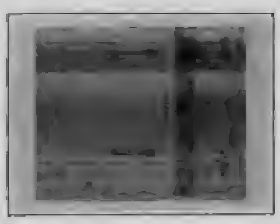
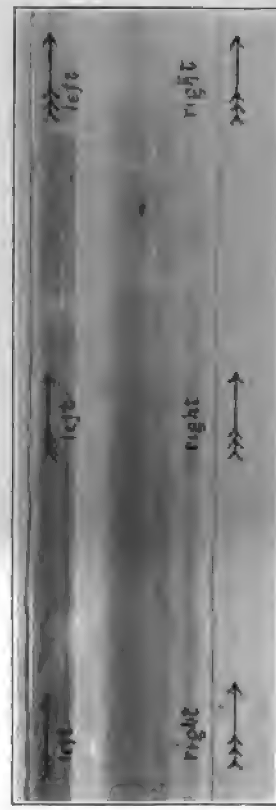


Fig. 3



(By an error Fig. 4 is shown reversed; the lettering is correct.)

Fig. 4

PLATE I.

corneal image showed on the negative as a dotted line in which the distance between any two dots represented one sixtieth of a second. Since the constancy of this alternation in the current has been measured in the Jefferson Physical Laboratory (of Harvard), and found to vary within a few tenths of one per cent only, the spacing of the dots on the negatives formed the most convenient possible means for determining the durations of the nystagmiform movements. These dots are shown in Figs. 3, 4, and 5 (Plates I and II).

Fig. 3 shows a portion of one of the films. The two curves are to be read from below upwards; but at the bottom is a photograph of the slit (showing a part of the subject's face) taken when the drum had made a little over one revolution and had come back to rest. Hence below the image of the slit, the curve of corneal reflection is doubled. "Right" and "Left" refer to the subject's right and left sides, so that the reader looks into the subject's face from in front. In the picture of the slit, the place on the cornea of the corneal reflection is shown; and also a minor reflection, which as may be seen traced no curve, from some other source of light. The fine line that crosses the slit horizontally is the image of the thread, above mentioned, which was used in adjusting the head. The time-dots are seen to be perfectly distinct, so that they could be accurately read with the help of a jeweller's eyeglass. Fig. 4 shows another part of the same negative, a portion subsequent to the single eye-curves of Fig. 3, that is, a continuation vertically upwards of Fig. 3. The rotation had been from the subject's left to his right, a direction that will be termed "clockwise" throughout this paper, and it can be seen that the quick eye-movements are toward the subject's *left*, while the slow are towards his right: had the photograph been taken ***during*** the rotation, the directions of the quick and slow movements would have been reversed. Two points may be observed in this figure which the tables will also bring out, — that the two eyes move together, and that as the nystagmus subsides the quick eye-movements become less frequent but endure no longer, or in other words, the slow movements alone increase in duration. The corneal reflection does not accurately show the amplitude of the movements; but direct inspection of a subject's eyes, as the nystagmus dies away, shows that generally (but perhaps not always) the amplitudes of both quick and slow movements decrease together. When this is the case, it follows that at the end of the nystagmus the ***rate*** of the slow movements decreases very much faster than that of the rapid movements.

Readable negatives were obtained from four, out of six subjects. Of such negatives there are fourteen, ten of which are of eye-movements after rotation clockwise, and four after rotation anti-clockwise. This distribution is accidental, for the rotations in each direction were about equal in number. With the exceptions to be noted later all the negatives exhibit the same features, so that of the fourteen only four examples are given in full in the tables; while for the others merely the averages of the duration of quick and slow eye-movements respectively are given.

TABLE I

Subject.	Film.	Eye.	Direction of the rotation.	Slow movements toward Subject's	Rapid movements toward Subject's	Average duration in seconds of slow movements.	Average duration in seconds of rapid movements.
C	1	left	clockwise	right	left	.32	.05
"	2	"	"	"	"	.36	.06
"	3	right	anti-clock	left	right	.26	.08
H	1	"	clockwise	right	left	.54	.07
"	2	"	"	"	"	.45	.07 }
"	"	left	"	"	"	.45	.07 }
"	3	"	"	"	"	.50	.08 }
"	"	right	"	"	"	.49	.08 }
"	4	"	anti-clock	left	right	.49	.07
"	5	left	clockwise	right	left	.53	.06
Ta	1	right	"	"	"	.73	.07
"	2	"	anti-clock	left	right	.48	.10
Tu	1	"	clockwise	right	left	.50	.06 }
"	"	left	"	"	"	.49	.07 }
"	2	"	"	"	"	.49	.12 }
"	"	right	"	"	"	.49	.12 }
"	3	left	"	"	"	.40	.07
"	4	right	anti-clock	left	right	.58	.08
					Av.	.48	.08

Table I gives these averages for all the fourteen negatives. In four of these (H 2, H 3, Tu 1, Tu 2) simultaneous curves for both eyes were obtained. In every curve the slow eye-movements were in the same direction as the previous rotation; the rapid in the opposite direction. The very few single movements that are exceptions to this are noted under Table II. Had the photographs been taken during (instead of after) the rotation, the directions of rapid and slow movements would undoubtedly have been reversed. It is

to be noted that when both eyes were recorded, their movements were generally identical, within the accuracy of measurement (one sixtieth of a second). There are a few exceptions to this. The averages of all slow and all rapid movements merely show that in general, and for that part of the nystagmus that was photographed, the slow eye-movements lasted six times as long as the rapid ones. This ratio varies considerably from one case to another, and at best throws little light on the whole nystagmiform series, since during the very first instants after the rotation the ratio of quick to slow movements would be less than one sixth, and at the very end of the series would be considerably more; this because toward the end the slow movements become much slower, while the rapid seem to change very little. The variations from case to case arise, at least partly, because in some cases the picture was taken more promptly, after the rotation stopped, than in others.

TABLE II

All records in seconds.

Subject C. Film 3. anti-clockwise.		Subject H. Film 2. clockwise.				Subject H. Film 3. clockwise.				Subject Tu. Film 4. anti-clockwise.	
right eye.		left eye..		right eye.		left eye.		right eye.		right eye.	
slow m. to lft.	fast m. to rt.	fast m. to lft.	slow m. to rt.	fast m. to lft.	slow m. to rt.	fast m. to lft.	slow m. to rt.	fast m. to lft.	slow m. to rt.	slow m. to lft.	fast m. to rt.
.26											
	.08	.03		.05		.06		.08			.06
.2			.51		.58		.1		.1	.45	
	.06	.06		.06		.06		.05			.08
.05			.16		.16		.13		.13	.75	
	.08	.05		.06		.1		.13			.06
.19			1.01		1.05		.36		.33	.36	
	.05	.06		.06		.1		.06			.13
.02			.26		.26		.28		.3	.48	
	.05	.05		.06		.13		.13			.05
.21			.26		.26		.28		.23	.61	
(.19)		.06		.06		.11		.13			.1
.16			.55		.6		.35		.35	.41	
	.05		.1		.1	.08		.1			.05
.03			.25		.25		.33		.3	.65	
	.05	.06		.05		.06		.11			.06
.18			.33		.33		.25		.2	.51	
	.06	.1		.1		.1		.1			.05
.03			1.65		1.65		.83		.83	.66	
	.05	.06		.06		.1		.13			.16
.29			.26		.26		.63		.61	.66	
	.11	.06		.08		.06		.06			.08

TABLE II, *continued.*

All records in seconds.

Subject C. Film 3. anti-clockwise. right eye.		Subject H. Film 2. clockwise. left eye.		right eye.		Subject H. Film 3. clockwise. left eye.		right eye.		Subject Tu. Film 4. anti-clockwise. right eye.	
slow m. to lft.	fast m. to rt.	fast m. to lft.	slow m. to rt.	fast m. to lft.	slow m. to rt.	fast m. to lft.	slow m. to rt.	fast m. to lft.	slow m. to rt.	slow m. to lft.	fast m. to rt.
.29			.78		.76		.45		.43	.68	
	.03		.06		.06	.05		.1			.15
.04			.16		.2		.45		.43	.23	
	.05	.1		.1		.06		.1			.11
.25			.38		.33		.4		.38	.36	
	.15	.08		.11		.08		.06			.08
.3			.58		.56		.58		.56	.35	
	.05		.06		.06	.06		.06			.06
.33			.78		.78		.58		.56	.38	
	.05	.08		.1		.08		.08			.05
.28			.71		.71		.35		.35	1.78	
	.11	.08		.06		.05		.06			.06
.41			.46		.45		.51		.5		
	.06		.05		.06	.06		.05			
.43			.56		.58		.6		.61		
	.11	.06		.06		.08		.1			
.35			.33		.31		.73		.68		
	.05	.1		.1		.06		.06			
.23							.86		.86		
	.15					.08		.08			
.38							.21		21		
	.1					.06		.05			
.43							.8		.81		
	.11					.08		.08			
.38							1.53		1.58		
	.03					.06		.05			
.53											
	.2										
.23											
(.18)											
.36											
	.03										
.23											
	.06										
.45											
	.11										
.25											
Averages											
.26	.08	.07	.45	.07	.45	.08	.50	.08	.49	.58	.08

Parentheses indicate time during which the eye did not move at all.

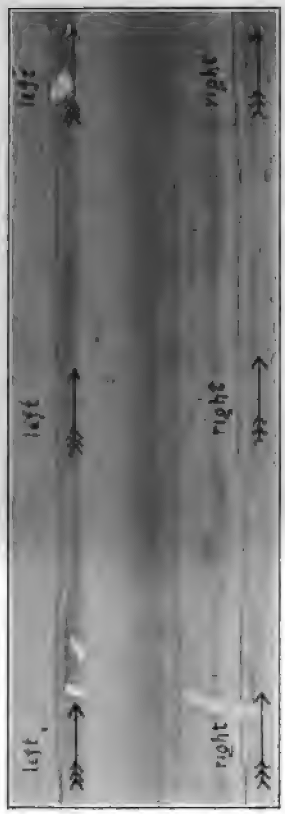


Fig. 5

PLATE II.

Table II gives in detail the data yielded by four of the most instructive films. C 3 is the longest record that was obtained; Tu 4 is among the shortest, though it is not the very shortest. H 2 and H 3 show how nearly alike are the simultaneous movements of the two eyes: .07 sec. is the greatest difference recorded on any film between simultaneous movements. All four records show how much less the duration of the slow movements is at the beginning of the record than at the end, and how little the fast movements vary in this respect.

H 2 is given because it is not typical; and about one half of the film itself is reproduced in Fig. 5 (Plate II). It will be seen that at four points there intervened between slow movements (toward the right) a rapid one that was also toward the right. This is the only record in which such a thing happened: and its explanation is problematical. With the subjects C and H, and only very rarely with these, a rapid movement sometimes took the place of a slow one, that is, occurred in the same direction as the slow movements (*e. g.*, Table II, C 3). And a trifle more often, yet very seldom, a rapid movement was relatively slow (*e. g.*, *ibid.*). With every subject there are a few cases in which the eyes stood still for a small part of a second (*e. g.*, *ibid.*), and these moments of rest seem to come after a rapid or a slow movement indifferently.

McAllister[1] and others have shown that the eyes are seldom at rest even when voluntary fixation is attempted, and these anomalies in the nystagmiform series may well be the result of such random factors, which instead of being always inhibited by the afferent impulses from the semicircular canals, which govern the nystagmus, operate along with these latter, and sometimes even inhibit them. With the exception of these anomalies, the movements recorded in the photographs confirm the observations of Purkinje, Mach, Breuer, Delage, and other investigators.

In conclusion, the sensations of vertigo and of nausea seem not to be essentially connected with the nystagmus. Several subjects were so disagreeably affected by a preliminary rotation that it seemed best not to continue the experiment with them. With those, however, whose eyes were photographed, while they experienced a mild degree of vertigo and nausea during and after the first few rotations, these sensations soon wore off with further practice, while so far as could be observed their eye-movements were as ample and

[1] C. N. McAllister: Yale Psych. Studies, Psych. Rev. Mon. Supplements, vol. 7, no. 1, p. 17, 1905.

VISION DURING DIZZINESS

BY E. B. HOLT

During and after a prolonged rotation of the head, the visual field seems to spin around before one's eyes, — a phenomenon that is ordinarily called the "dizziness of Purkinje." Delage describes it as follows:[1] "In the experiment of Purkinje, while we are rotating in a positive sense, space seems possessed of a motion in the opposite direction. . . . This phenomenon is explained *by the direction of the nystagmus.*"

"In the nystagmus," he continues, "the eyeballs execute two well-differentiated motions: one, a compensatory, *relatively* slow motion, during which images pass across the retina so as to give the appearance of a movement of space in the opposite direction; two, a swift motion opposite to the slow one, and so rapid that the images passing across the retina leave no sensation of their movement."

Now, in a previous paper[2] I have shown that there is a central anæsthesia, or central inhibition of visual sensations, during about the latter two thirds of the time occupied by every voluntary eye-jump; and in view of this I was led to enquire whether in fact, as Delage so confidently asserts, it is the *speed* of these more rapid movements, or some other factor, that causes them to leave no visual sensations. There can be no doubt that they do leave none, since, aside from the statement of Delage, in dizziness the visual field whirls always in only one direction; whereas it should otherwise appear to swing now to one side, now to the other, as the eyes move back and forth across the objects. I have found but one other mention of this point in the literature. In his Analyse,[3] Mach

[1] Yves Delage: Physiol. Studien über d. Orientirung (Aubert's transl.), p. 100, Tübingen, 1888.

[2] E. B. Holt: Harvard Psych. Studies, Psych. Rev. Mon. Supplements, vol. 4, p. 1, 1903.

[3] E. Mach: Analyse der Empfindungen, 2d ed., p. 98, Jena, 1900.

says, parenthetically, "(the jerky eye-movement leaves no optical impression)"; but he does not suggest that this is because of its greater speed.

In order to test this point, a 2 c. p. incandescent lamp was so arranged that it could be moved vertically in front of, and about four metres distant from, a rotating chair. Since after a rotation the eyes are oscillating from side to side, if the lamp is moved up and down an obliquely inclined after-image streak must be generated on the retina; and clearly there are four possible positions in which this may lie, as shown in Fig. 1.

The results were absolutely uniform (the author alone as subject); the after-image streak always lay on that side of the moving light *toward* which the *slow* eye-movements were directed, that is, the lamp appeared to drift obliquely up or down and in a lateral direction *opposite* to that of the slow eye-movements. Apart from its vertical displacement, then, the lamp behaved like the less intensely illumined parts of the visual field, seeming to be totally invisible during the swifter eye-movements. Now since the experiment was done in a partially darkened room and the eyes were partly adapted to darkness, the lamp should have been intense enough adequately to stimulate the retina even during the more rapid movements, and might be expected to leave an after-image streak on that side toward which these rapid movements were directed, and differing only from the streaks seen during the slow eye-movements in being inclined at a less angle from the horizontal. Yet no such streaks were visible.

These observations were made at about the same number of seconds after the rotation stopped, as the photographs were taken that are recorded in the preceding paper of this volume. The rapid movements were therefore about one sixth as long in duration as the slower ones. Since the respective amplitudes of rapid and slow must average very nearly the same, the rapid movements must have been about six times as swift as the slow movements. It needs therefore to be shown beyond a doubt that the 2 c. p. lamp *was* bright enough, in view of the briefness of stimulation of any one retinal element during the rapid eye-movement, to be above the threshold of perception. For this reason the experiment was not continued with other subjects.

The certainly adequate degree of illumination was realized during the photography of the eyes described in the preceding paper. Here during the post-rotary dizziness an arc lamp (of 6 amp.) was in

front of the face and but a little to one side of the primary line of regard; it was 60 cm. distant from the eyes and on a level with them; a lens condensed the rays on the two eyes, and the light was diminished only just enough as not to be painful, by a dilute

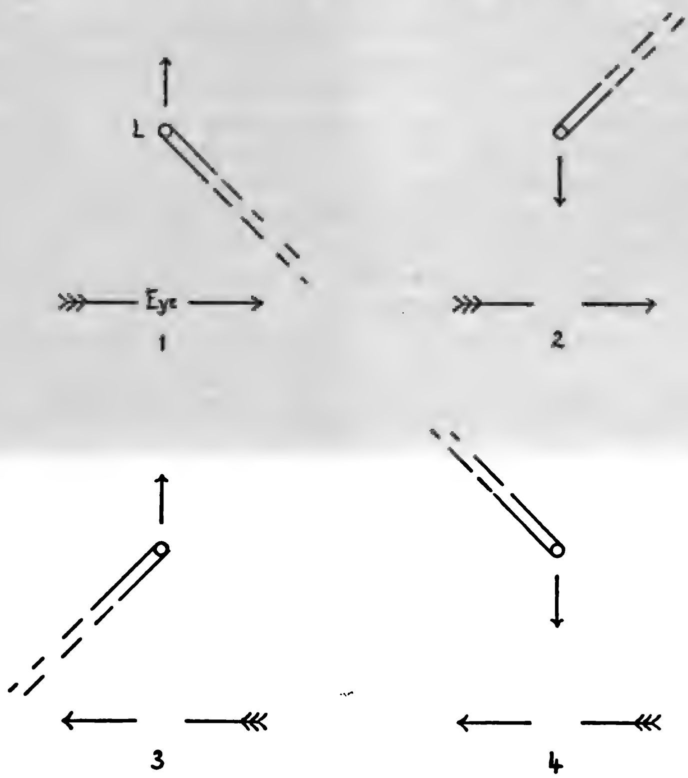


Fig. 1

screen of copper ammonium sulphate about 3 cm. thick. Of course such an illumination must adequately stimulate each retinal element even during the most rapid eye-movements. Nevertheless with the four subjects that were photographed the arc lamp, like the rest of the visual field, seemed always to swim in one direction, and that opposite to the slower eye-movements. In one case where the eyes were photographed without the adiathermal screen, and the light was rather painfully intense, the lamp was still seen to drift in one and the same direction. There was never any trace of its moving to and fro, as there should have been had it been visible during both phases of the nystagmiform movements.

This absence of visual sensation during the more rapid eye-movements might conceivably depend on either peripheral or central inhibitory factors. But the anatomy and physiology of the eye offer no point of support for the supposition that during such movements the irritability of the rods and cones is momentarily reduced, or that the retinal layers posterior to the rods and cones suffer an interruption of function during a movement of the eyeball in its socket. Indeed, during some such movements, the "pursuit" movements (Dodge's second type), vision is unimpaired.[1] In view of these facts, and of the many known cases of the mutual inhibition of sensations where undoubtedly the process is a central one, it is by far most probable that this visual inhibition is also a central process; as was certainly the visual inhibition during voluntary eye-jumps, previously reported by me.[2]

The conclusion above reported that the visual inhibition during the more rapid phase of the nystagmus in no wise depends on an inadequate stimulation of the retina, due to the greater speed of the rapid movements, and that the inhibitory process is purely central, is further supported by the following phenomenon. If before the rotation has commenced, the eyes are so strongly stimulated that a lasting after-image is obtained, this after-image will, during the rotation, always be seen to swim in the direction opposite to the rotation, that is, *with* the *slow* eye-movements; but when the rate of rotation begins to decrease, and as Mach, Breuer, and Delage have shown, the slow eye-movements reverse their direction, the after-image also reverses its direction, and now swims in the direction of rotation, that is, *still with* the *slow* eye-movements. If the after-image persists long enough, it may still be observed, after the rotation has ceased, swimming in the same direction as the surviving slow eye-movements. If, for instance, the slow movements are from left to right, the after-image (best seen with the eyes closed) swims from the left to the right hand side of the field and disappears, reappears at the left and swims again toward the right, and continues to do this until the nystagmus entirely ceases.

This experiment was repeated several times, with four subjects, and with both clockwise and anti-clockwise rotations, and the results were uniformly as described above. In order to see whether this motion of the after-image really depended on the slower ny-

[1] R. Dodge: Amer. Jour. of Physiology, vol. 8, p. 317, 1903.

[2] E. B. Holt: Harvard Psych. Studies, Psych. Rev. Mon. Supplements, vol. 4, p. 42, 1903.

stagmiform movements, the following variation was tried. It will be recalled that if the head is rotated not about a vertical (longitudinal) axis, but about a transverse axis, as, say, one passing through the ears, a nystagmus is produced in which during the rotation the slower eye-movements are opposite to the direction of rotation, while when the rotation is checked or stopped, the nystagmus, as before, reverses. The same is true if the rotation is about a sagittal axis. These conditions were approximately realized by having the subject sit as before on the rotary chair, but during the rotation hold his head horizontally to the right or left, forward or back. With any of these positions of the head, however, the rotation produced, on all of the subjects tried, extreme dizziness and a feeling of nausea that lasted in some cases for several hours. This fact made it impossible to ask for a set of the four possible positions of the head from any of the subjects. The following are the records that were obtained:

Subject Fl.	Head horizontally to left; rot. anti-clockwise.	
	During rot.;	after-im. moved clockwise, *i. e.*, from subject's brow to chin.
		Eye-mov. not observable during rot.
	After rot.;	after-im. moved anti-clockwise, chin to brow.
		Slow eye-mov. anti-clockwise, chin to brow.
		Vis. field clockwise, brow to chin.
Subject H.	Head horizontally to right; rot. anti-clockwise.	
	During rot.;	after-im. clockwise, chin to brow.
		Eye-mov. not observable.
	After rot.;	after-im. anti-clockwise, brow to chin.
		Slow eye-mov. anti-clockwise, brow to chin.
		Vis. field clockwise, chin to brow.
Subject H.	Same repeated, with same results.	
Subject H.	Same as case of Fl., with identical results.	
Subject K.	Head horizontally to left; rot. anti-clockwise.	
	During rot.;	after-im. not observed.
	After rot.;	after-im. anti-clockwise, chin to brow.
		Slow eye-mov. anti-clockwise, chin to brow.
		Vis. field not observed.

So far as these records go, they entirely confirm the results of other investigators as to the direction and the reversal of the nystagmus. In each of the cases the after-image moved with the slow eye-movements, reversing its direction with these slow movements, while the visual field whenever it was observed (the eyes were kept

closed during the rotation) moved in the opposite direction to that of the after-image and the slow eye-movement. It is well known that after-images move *with* every involuntary eye-movement, and although they disappear during voluntary eye-jumps,[1] they reappear at the end of the jump in a position that is related to the new fixation-point exactly as the old position was to the former fixation-point. These after-images, then, are seen during the slow eye-movements whose direction they follow; but are not seen during the quick movements, when they must naturally move in the direction of these quick movements. And aside from this it is possible to observe introspectively that the after-image disappears at that side of the visual field toward which the slow eye-movements tend, and is for a moment invisible before it reappears on the other side of the field. As was shown above, the visual field always moves opposite to the direction of the slow eye-movements, as must of course be the case if there is no inhibition of vision during these movements. The simultaneous appearance of the after-image moving with, and the rest of the visual field moving contrary to, the direction of the slow eye-movements, with a uniform absence of the converse phenomena, seems to prove that vision is unimpaired during these slow movements, while it is completely inhibited during the rapid phases of the nystagmus.

Purkinje himself[2] called the slower phases "involuntary and unconscious," meaning by "unconscious" not that the visual field was not seen (for it just then is seen), but that the movement of the eyeball during the slow phases was not felt. I have observed, with the confirmation of several subjects, that *this* movement can also not voluntarily be inhibited; whereas the swift movement is so far voluntary that it can be inhibited at pleasure. It is possible, that is, to fix the eyes on that side of the field toward which the slow movements are directed, but not on any point at the other side of the field. The slow movements, then, during which vision is possible, are purely reflex. These slow movements, purely reflex and yielding clear vision, with the rapid movements, partly under voluntary control and attended by an inhibition of vision, present a parallelism, that may be not without significance, to the

[1] S. Exner: Zeitschrift für Psych. u. Physiol., vol. 1, p. 46, 1890; E. Fick and A. Gürber: Berichte d. ophth. Gesellschaft in Heidelberg, 1889; E. B. Holt: *op. cit.* p. 4.

[2] Purkinje, 1825; reprinted in Aubert's Physiol. Stud. über d. Orientirung, p. 117, Tübingen, 1888.

" pursuit " eye-movements (Dodge's " second type "), that are likewise relatively slow, are reflex, and yield remarkably clear vision, and the ordinary voluntary eye-jumps (Dodge's " first type "), that are relatively rapid, and are, like the rapid nystagmiform movements, attended by a central inhibition of vision.

VISUAL IRRADIATION

BY FOSTER PARTRIDGE BOSWELL

THERE are various kinds of visual irradiation, of which perhaps the best-known variety is that which appears as the enlargement of a brightly illuminated surface at the expense of a contiguous one of less intensity. This has been until recently the only form recognized, and until very lately the greater part of the literature has dealt with it alone.

The whole subject was carefully investigated by Plateau in 1831, and retinal irradiation extricated from phenomena which very often accompany it. He showed that the extent of irradiation varies with the intensity of the stimulating light and the time during which it is allowed to act. He was also the first to call attention to the phenomenon of so-called negative irradiation.

Somewhat later Volkman again called attention to negative irradiation, while Aubert, in opposing the explanation advanced by Volkman, first showed the relations existing between irradiation and contrast.

Dove was the first to investigate the influence of irradiation on stereoscopic pictures, thus calling attention to the question of binocular irradiation. Experiments in this direction, however, have in general given negative results in so far as any enlargement of the binocular portion is concerned.

Helmholtz examined the manner in which the stimulation at the border-line between a light and dark field changes in intensity, and drew a curve showing these modifications of intensity due to irradiation. Hering showed that the form of the Helmholtz intensity curve would be modified by the presence of other phenomena not strictly those of irradiation.

De Roux demonstrated the difference in the extent of a real induction on the foveal and the extra-foveal parts of the retina.

Charpentier has attempted to carry forward the general explanation by saying that this spreading of neural excitation, the existence of which he proves to be beyond question, takes the form of an undul-

atory excitation in the free nerve-endings of the retina. Bidwell has investigated more thoroughly in some respects than Charpentier the phenomena of the after-images of moving sources of light, which have bearing upon irradiation. The same is true with regard to McDougall, von Kries, Hess, and others. Burch has instituted investigations along these lines, especially concerning the inhibition of stimuli on contiguous portions of the retina. Hess has worked carefully upon the different phases of the stimulation derived from a moving source of light, the differences in functioning of the foveal and extra-foveal parts of the retina, the respective functions of the rods and cones, and in connection with this, made investigations in the visual perception of color-blind subjects. All these observations have important bearing on irradiation, contrast, and theories of color-vision.

In connection with some work which was being done upon the after-images of moving sources of light in the Harvard laboratory in the early winter of 1903, some phenomena were observed which I believe are due to one form or other of visual irradiation. They can be seen in various ways, perhaps most advantageously by observing with fixed eyes the passage of a luminous image over the retina. What one sees as such a figure moves by is a travelling band of light, its forefront somewhat like that of the stimulating source, the rest composed of a long train of after-images which differ very decidedly from one another in intensity and color. The advantage of this well-known method of observation lies in the fact that it enables one to translate the temporal relations between the different phases of the stimulation into spatial relations between the different portions of the moving band of light. For since the figure moves across in a plane before the observer, that which appears in his consciousness first in time will likewise appear as foremost on the plane in space. Thus by observing the train of images one practically sees the different phases of the stimulation spread out in order before one. The new phenomena we observed, however, have to do with but a single phase of the stimulation, the extreme front of the stimulating image.

The intensity of light used varied considerably with the differently colored images, and was regulated so as to give as well as possible the phenomena we wished to study. With white light the intensity was less than that of an eight-candle-power electric lamp placed about ten feet distant from the observer. When colored light was employed it was necessary to use a very much stronger source of illumination, since the colored glass which was used absorbed a great

deal of light and in case of colors lying toward the violet end of the spectrum greater luminosity seemed demanded.

The apparatus used consisted of a three-foot pendulum with a screen attached. This screen swung with the pendulum. In the screen was an opening about four inches wide and three inches high, into which strips of cardboard or tin backed by a piece of ground glass could be slipped. In these strips differently shaped holes were made through which the light passed. In this manner an image of any desired form might be used. Behind the screen, between it and the lamp, was a frame in which other pieces of ground or colored glass were placed. These pieces of ground glass would reduce the intensity of the light and diffuse it evenly over the image. The observer sat ten feet away. When the pendulum was set in motion, the image would appear moving back and forth in an arc. In order to shorten this arc and to aid the observer in keeping his gaze perfectly fixed, a second screen was placed before and very close to the pendulum, between it and the observer. This screen was stationary. In it was a hole six inches long and two inches wide. The top and bottom of this hole were arcs of circles parallel with the arc in which the pendulum swung. The ends were radii.

The screen was so placed with reference to the observer that the moving image would pass directly across the middle of the opening, appearing from behind one side and disappearing behind the other. In the centre of the opening, directly in front of the place occupied by the moving image when the pendulum was at rest, were two luminous fixation-points, one above the other, below the path of the moving light. In order to measure apparent spatial differences between the phases of the stimulation, two wires were stretched vertically across the opening in the stationary screen. These wires could be moved nearer together or farther apart. Thus by measuring the apparent distances in space between the different parts of the moving figure a measure could be had of their temporal differences in coming into consciousness. The luminous image moved, during the time it was visible, at a velocity of about one and a quarter feet per second. Since the observer sat about ten feet from the instrument, this would be at an angular velocity of about seven degrees per second. In one experiment a higher and a lower velocity were also employed.

It was of course very easy to change the figures and vary them widely in form, color, and intensity. Most of those employed, however, were rather small, subtending an angular distance of not more than one degree. Since the whole opening did not subtend an

angle of more than three degrees or so, nearly all the phases of the stimulation occurred at the fovea.

We noticed that the form of the stimulating images themselves seemed to suffer modification as the light swung by, not only because of the train of after-images which dragged behind them over the retina, but in other ways as well. For instance, a circular image (Plate III, Fig. 1) appeared crescent-shaped, and its forward edge possessed greater curvature than the segment of the circle which produced it. It was longer also from horn to horn than the diameter of the generating circle, and a faint haze surrounded the points extending outward and backward until lost in the blackness of the background. Von Kries remarks that a circular moving image appears cylindrical in form with a concave edge behind. By using a little higher speed we observed this phenomenon. At first we thought the crescent-shaped image to be due merely to an intensely black after-process, which Bidwell describes as following the positive image of a bright white light. This, taking place before the circular disc of light had gone forward a distance equal to its own diameter, would overlap the bright image from behind and a crescent-shaped figure would result, but the increase in width and convexity of the stimulating image as well as the laterally trailing clouds of light remained to be explained, and as this could not be done in terms of anything which might happen to the back of the image, another explanation had to be sought. In order to determine the effect of the form of the figure used as a source of light on the form of the apparent image, several differently shaped figures were employed. In place of the original circle, an oblong pointed at both ends was tried (Plate III, Fig. 2). The front of this figure appeared very convex indeed, while the ends, which, owing to the shape of the figure, were very much less effective as a stimulating source, trailed far behind the centre.

A crescent-shaped figure (Plate III, Fig. 4) gave rise to a very pretty phenomenon. When it moved toward its concave side, it appeared very much less concave on that side than the real figure, but when it moved the other way, toward its convex side, it seemed very much more curved than it was in reality.[1]

[1] Image no. 5 appeared with the concavity in front. In the centre of the figure appeared a dark grayish splotch of light, very much darker than the rest of the image. This is due, most probably, to the presence of Charpentier's phenomenon of recurrent bands. If this happens in this figure the ends of the recurrent bright images would overlap while the centres would not, so that the black bands appearing, as it were, through interstices in the central part of the figure, would seem like

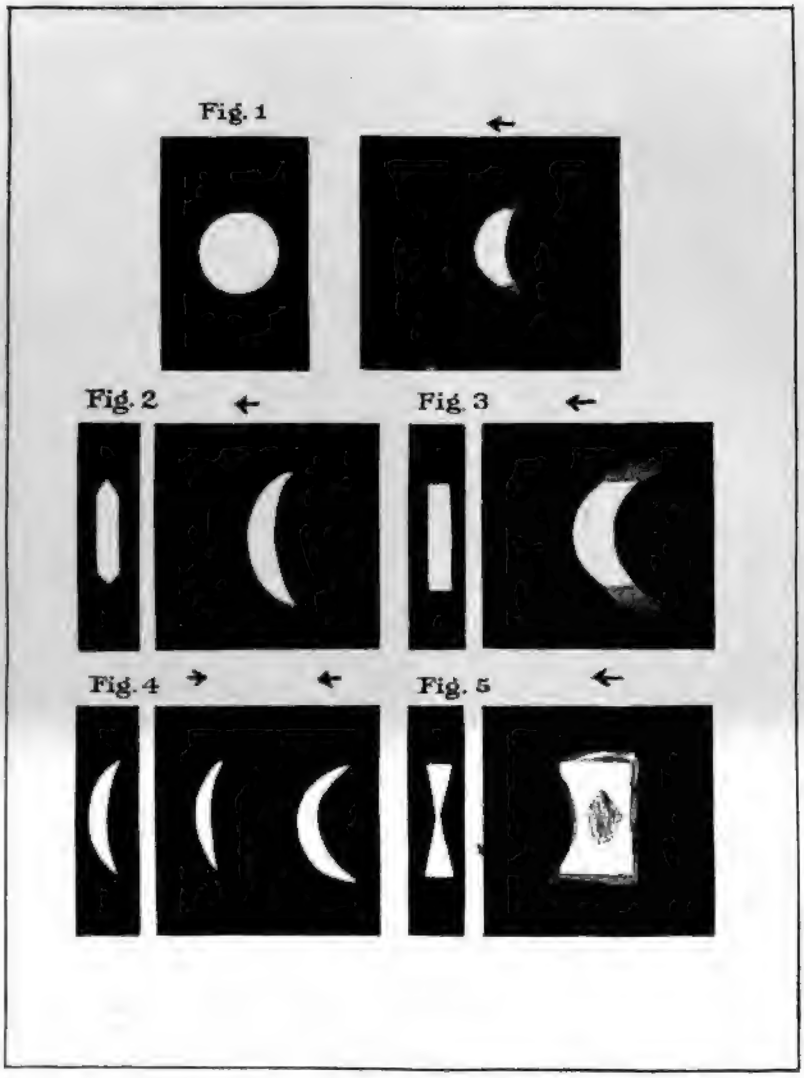


PLATE III.

No. 3, a simple oblong figure, appeared curved like the others, almost as perfect a crescent as any of them.

The idea occurred to me that perhaps all these modifications in the curvature of the figures could be explained if we assumed two things: First: that there is a spreading of excitation from one portion of the retina to another. Each point will therefore be stimulated not only by the light falling directly upon it, but it will also derive a certain reënforcement of its stimulation from the points surrounding it. Thus a point lying toward the centre of one of these figures would be more favorably situated for receiving reënforcement than one located toward the periphery, where there are few neighboring points, and those lying mostly in one direction, namely, toward the centre.

This may be represented diagramatically, as in the illustration (Plate IV, Fig. 10), where the horizontal coördinates represent the spatial dimensions of an oblong image and the vertical coördinates the intensity of the excitation due to direct stimulation and its reënforcement by surrounding points at various portions of the figure.[1] Secondly I assumed that the stimulation at one part of the figure being thus rendered more intense, that part would appear in consciousness more quickly than the others and cause a modification in the form of the figure.[2] For example, in the case of the oblong figure, the light would be rendered most intense at the centre and less and less intense toward the ends, for the points in the centre of the figure will have their intensity increased by nervous excitation spreading to them from points lying toward the ends. Those toward the ends will be reënforced by light coming only from toward the centre. Thus the intens-

a dark splotch, especially since the outlines of the bands are vague and hazy. The back end of the figure had the effect of being vertical or nearly so. This is probably due to the same cause as that which made the circular figure to appear as it did, namely, the negative after-image overlapping the positive after-image. The front of this black image is usually of about the same shape as the front of the real figure which it follows. If this is so, then, in this case, it would make the back part of the image pretty nearly vertical.

[1] The intensity of the objective stimulation may be represented by the line *AB* If there were no reënforcement of stimulation the whole figure would be flat on top and of this height. The difference between *AB* and *AC*, or *BC*, represents the increase of intensity due to irradiation at the most favorably situated portion of the figure. The other portions receive increments proportional to their location, as indicated in the diagram.

[2] Favorable localization will of course depend largely on the shape of the figure in which the point is situated. Thus one in the angle of a triangle or at the horn of a crescent would have much less reënforcement of excitation than another point, say halfway down the side.

ity of the centre of the figure will be increased, and as the figure moves across before the observer, the centre, appearing first in consciousness, would likewise appear foremost in space, the points near the centre a little later and so on, until finally, the ends being the last to appear, the whole front of the figure would take the form of a convex curve, after the manner in which it was observed. The back of the figure also appears curved, probably because of the fact that the front of the negative after-image, which closely follows it, is of the same shape as the front of the positive image, as was shown in the case of the circular figure.[1]

It is of course a well-known psychólogical fact that a light of greater intensity will take less time in coming into consciousness than one of less intensity. In this case, however, it was necessary to find some way of showing such differences between lights which were very little different in intensity. For one is practically unable to see any difference in intensity between the parts of a stationary image. So unless it could be shown that a difference in intensity between two sources of illumination, so small as to be imperceptible to the observer, will nevertheless make its presence known by the appearance of the brighter light in consciousness before the other, the explanation which I have suggested for the curvature of the images would have to be abandoned.

The following experiments do show, as I believe, that of two sources of light not perceptibly different in intensity, the brighter will appear in consciousness before the other, and that in the case of these figures the curvature of the image is due to a heightened intensity of the light in the centre through reënforcement of the excitation there present by stimulation spreading from the ends.

Experiment I

In the first of these experiments three dots of about three sixteenths of an inch were placed in a vertical row about three eighths of an inch apart (Plate IV, Fig. 1). No change was then observed in the form of the figure. The row of dots swung across the opening in a perfectly vertical line one directly above the other (Plate IV, Fig. 2). They were

[1] Bidwell describes this "black process" or "negative after-image" of a bright, white light as being of a blackness more intense than the ordinary blackness of an entirely dark room. This is perfectly true. The black image, however, lasts for a very much longer time than the recurrent images of the same light. Often this velvety black band would trail along behind the moving light for the distance of a foot or more, gradually lightening into the darkness of the background.

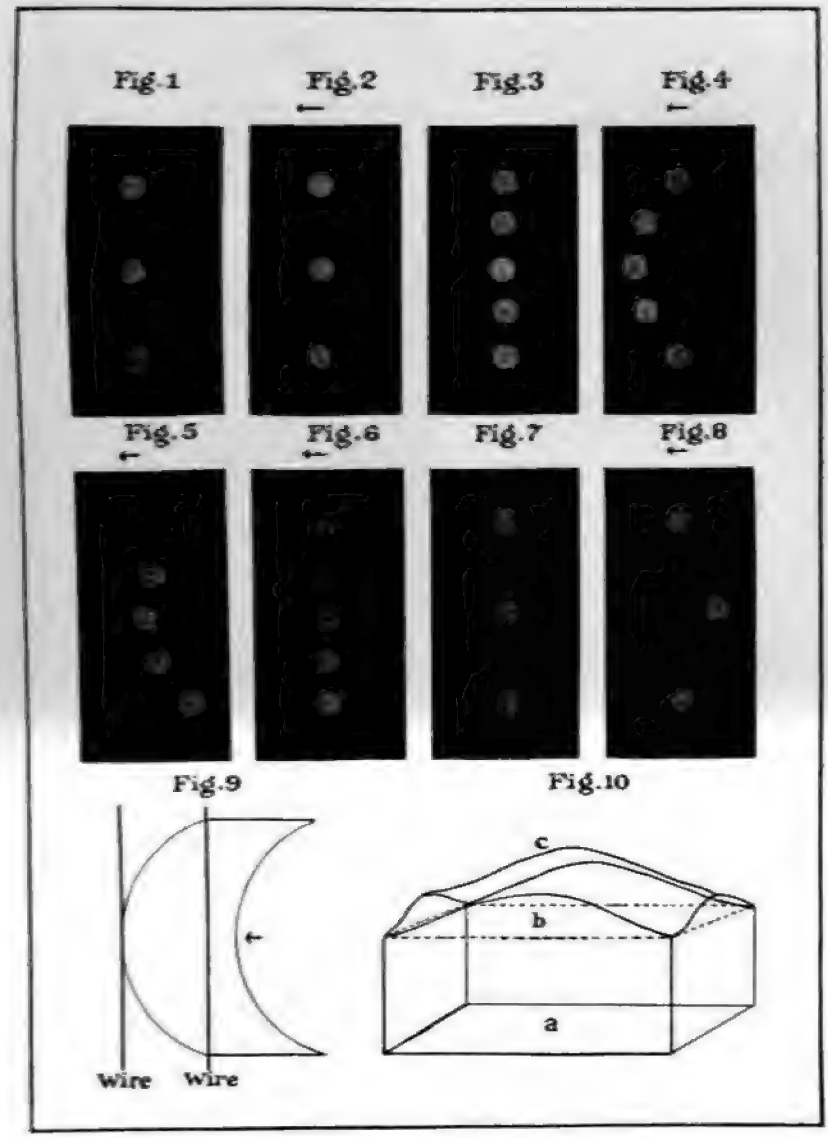


Fig.1
Fig.2
Fig.3
Fig.4
Fig.5
Fig.6
Fig.7
Fig.8
Fig.9
Fig.10
c
b
a
Wire
Wire

PLATE IV.

presumably too far apart for irradiation to take place between them. When, however, another dot was interposed between each end dot and the centre dot (Plate IV, Fig. 3), so that the excitement could extend from one dot to the next, the front of the line of dots no longer appeared vertical, but decidedly convex, the centre dot being perhaps three eighths of an inch before the dots on the ends (Plate IV, Fig. 4).

Absolutely the only difference between the two cases was that in the one, irradiation presumably could not occur, while in the other it conceivably could.

Experiment II

In the second of these experiments the curvature of a line of dots was observed and measured. Then the centre dots were slightly darkened (Plate IV, Fig. 5) by shading lightly with a lead pencil the ground glass which travelled with the pendulum and held in place the card from which the dots were cut, until the front of the image lost its curvature and appeared vertical (Plate IV, Fig. 6). The pendulum was then stopped and the row of dots observed closely, in order to see whether the dots in the centre were perceptibly of less intensity than those on the ends. No perceptible difference was found.

Experiment III

All the dots were covered, except the shaded central and the two unshaded end dots, in order that no irradiation might take place between them (Plate IV, Fig. 7). The pendulum was again set in motion, and the centre dot, instead of remaining co-linear with the dots on the ends, appeared considerably behind them (Plate IV, Fig. 3). This would show that irradiation must heighten the intensity of the excitation in the centre of the figure — for the two cases just mentioned are alike in every respect except that in the first (Fig. 6), where the dots were near enough together so that irradiation might occur between them, the intensity of the centre dot, which was objectively fainter than the end dots, was heightened enough by this induced excitation to appear in consciousness as soon as the two end dots, which were objectively of greater intensity; whereas in the second case (Fig. 7), where the dots were too far apart for irradiation to take place between them, the centre dot, being objectively of less intensity than the end dots, appeared behind them.

These experiments show that of two sources of light very little different in intensity the brighter will appear in consciousness before

the other. Other things being equal, the difference in intensity may even be so small as to be imperceptible by direct comparison; it is able nevertheless to make its presence known by the order in which the lights appear. Exner made some experiments in 1868 to determine the time necessary for the perception of lights of different intensity. He used, however, stationary images of brief duration and tried to eliminate the effects of the after-image by flooding the visual field with light. This method has its disadvantages. It is incapable of measuring the minute temporal differences in latent perception of sources of light very slightly different in intensity.

While my method does not give the absolute time taken by any one light to enter consciousness, it is a very much more delicate method than Exner's for measuring *differences* in time of latent perception of sources of light very close to one another in intensity. It would be a very easy matter, having found the time of latent perception for a light of standard intensity, to determine by this method the time of lights of greater or less intensity.

These experiments also show that when irradiation is absent, the curvature of the images is absent; when irradiation is presumably present, curvature is present. For I find, not only in these, but also in a number of other experiments, that under all conditions in which the presence of irradiation is to be expected, the form of the images tends to be modified in precisely the manner that the assumption of its presence would lead one to anticipate. In all cases where irradiation is presumably absent, the contour of the front of the moving figure depends entirely on the amount of light proceeding from its different parts.

It is next in order to say something of the physiological causes of the phenomena we have been considering.

It is probable from what has been observed that in the case of the curved figures we are dealing with a form of visual irradiation which is due to the spreading of neural excitation over or through the layers of the retina. It is also evident from the close connection between irradiation and intensity that it must be of such a kind that the excitation produced in one part of the retina may communicate itself readily to another part. We have also seen in the case of the moving line of dots that the several dots could remain distinct from one another and yet could reënforce each other by means of communicated excitation. It must also be a very rapid form of irradiation, for the curvature of the figures does not increase very much during the time they are visible.

I think that the demands made by these different facts are best met by assuming that the spread of the nervous excitation which gives the reënforcement takes place in one of the interconnecting layers of nerve cells and fibres underlying the rods and cones. The line of dots which appeared curved and yet perfectly distinct from one another could very well communicate excitation to one another along these fibrils, and the intensity of one part be raised by the excitation of the near-lying parts. The fact that the dots remain distinct would not be contradictory. For in that case very near-lying parts might communicate excitation to one another without arousing to any very great activity the nerves that lead to the brain from the small unstimulated portions which lie between them. In this manner the intensity of the centre dots could be heightened enough to make the row appear convex, without any merging into one another on the part of the several dots. The fact that the dots do not fuse shows that the curvature is not due merely to a forward-spreading of the excitation in the retina. However, there is always a certain amount of light visible between the dots, with all the colors. This is especially noticeable with green light.

The fact that the elements of the retina form a kind of concatenated series from without inwards, a number of rods and cones corresponding to but one ganglion cell, furnishes a further bit of evidence in support of the explanation just advocated, since the irradiated excitation would tend to be "drained off" through the group of ganglion cells corresponding to the most highly stimulated portions and leave the intervening spaces comparatively free from centrally proceeding excitation. Thus also the individual dots in the five-dot figures may appear entirely distinct from one another and yet the centre ones be reënforced enough by irradiation to appear in consciousness in advance of the others.

SUBSIDIARY EXPERIMENTS

A number of other observations were made which present various exemplifications of the principles we have considered.

Experiment IV

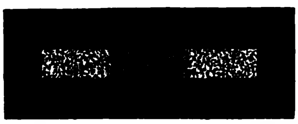
An oblong figure, all its parts objectively of the same intensity, had its ends slightly darkened. When this was done the curvature had increased from twelve sixteenths to fourteen sixteenths of an inch.

The pendulum was stopped, and a very slight difference was per-

ceived between the ends and the centre of the figure. This difference in intensity was greater than in the dot experiment, when the image had been darkened enough in the centre to make it appear vertical, because in this case, when the ends were darkened the centre would still be reënforced by irradiation from a considerable space which intervened between the shading and the centre.

Experiment V

The centre of the oblong figure was considerably darkened so as to counteract the effect of induction. By properly varying the amount of shading, one may make the front of the figure appear less convex, vertical, or even concave. This shows perfectly the effect of differences in intensity upon the curvature of the figure, but does not show so neatly as the similar experiments performed with the dots, the influence of the presence or absence of irradiation upon the intensity of the centre of the figure and so upon the curvature.



The illustration shows a case where the centre was too much darkened.

The two ends were comparatively free from shading. In each end-part irradiation took place. The points lying toward the centres of these ends received reënforcement, both from points lying toward the centre of the figure and from the extreme ends, and so the centres of the ends of the image were considerably brighter than either the extreme ends of the figure itself, or the sides of the end-parts toward the heavily shaded centre of the figure. Accordingly each end appeared convex for a short distance. The whole figure, however, being considerably brighter at the two ends than at the centre, on account of the heavy shading, the ends appeared in consciousness first and the centre afterwards, so that the figure as a whole seemed concave.

Experiment VI

An oblong figure was shaded rather heavily at one end, gradually becoming lighter toward the other, while about a third of the figure

was free from shading. The shaded end always seemed to lag behind. The extreme front of the figure was at a point a little distance from the other end, before the shaded portion began. So that the front of the whole figure appeared, not like a segment of a circle, but like part of an oval with the bulge toward the brighter end.

Beyond the ends of all these images faint clouds of light were seen, as has been mentioned before, extending outward and backward, gradually decreasing in intensity, until lost in the surrounding blackness of the background.

Charpentier's bands, sometimes more and sometimes less in number, were observable in all of my figures and with all colors. Very often they appeared to be parallel to the forefront of the image, or even of a slightly greater degree of curvature.

Experiment VII

It is a well-known fact that a rotating color-disc, having colors which just fuse at a certain intensity, will show flicker at a slightly less intensity.

A color-disc was set in motion and the speed found where the colors were on the point of fusing. A piece of black cardboard, with a hole about an inch in diameter, was held close to the screen.

Around the periphery of the hole flickering appeared, while at the centre there was fusion. (The cardboard was held very close to the disc, so that there would be no shadows on the disc near its edges.) This fusion at the centre of the disc is probably due to the fact that the centre of the field is of slightly greater intensity than the edges, owing to irradiation. This difference in intensity makes the difference between the fusion at the centre and the slight flicker seen at the periphery.

Karl Marbe in a recent article mentions the difference in fusion between a point in the centre of the disc and a point near its border, and he thinks the increase of flickering in the latter is due to some influence on the part of the moving edge which separates the different parts of the disc. It would seem more probable from this last experiment that the fusion at the centre of the field of view was due to reënforcement of intensity by irradiation, and that the flicker about the periphery of the field was due to the lack of such reënforcement.

Experiment VIII

Three large dots were used and the centre one covered with tissue paper. The two end dots then appeared ahead of the centre dots.

They were larger than the centre dot, due to irradiation over their borders. But this increase in size did not account for their position ahead in space. The centres of all the dots were not co-linear, but the middle dot was behind the others, thus, of course, showing the greater time necessary for the perception of the less luminous object.

Experiment IX

This was exactly similar to the preceding, except that the intensities of the various dots were reversed. The end dots were covered with tissue paper, instead of the centre one. Then the centre dot appeared first and the end dots after it.

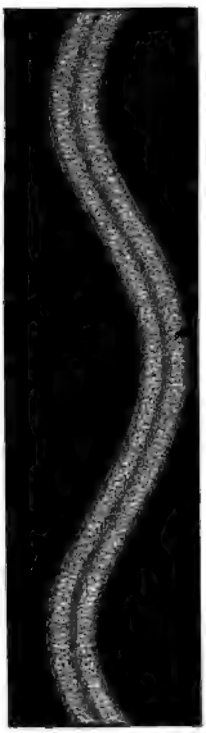


Figure observed with centre curved backward at the fovea, and ends curved forward owing to irradiation

Experiment X

Professor Hess finds that an image which, compared to those we used, was very long, subtending an angular distance of about thirty degrees, and which extends entirely across the fovea and overlaps the surrounding parts of the retina will appear curved backwards at the fovea, owing to the longer time of latent perception of the fovea and the macula. The accompanying illustration shows a modification of one of Hess's figures, in which the presence of this phenomenon and that of the convex image are both shown. The two phenomena were observed when a two-inch image was observed at a distance of about fourteen inches. The intensity of the light was that of an eight-candle-power lamp with three pieces of ground glass in front of it. (Very many of Hess's intensities are too great to give convex images.) Thus the image would be about 12° in height. About $\frac{5}{12}$ of the figure would then fall on the macula and fovea and appear curved backwards in relation to the ends. The ends where they fell on the extra foveal parts of the retina appeared convex in front and concave at the rear as any small image of the right intensity does which falls on a homogeneous part of the retina.

Experiment XI

Charpentier, Bidwell, and others have made the observation that if a small source of light be exposed for a brief interval, excitation will proceed out in all directions over the retina, but if the light be exposed for a slightly longer period, the excitation will contract again and the light appear nearly its proper size and in its proper location at the stimulated portion of the retina. Using variously shaped figures we obtained analogous results, and the additional fact appeared that the outgoing excitation proceeds from the borders of the figures and that its form is somewhat determined by the form of the figure. An oblong image appeared vaguely elliptical, a diamond-shaped figure in the form of a more pointed ellipse, etc. These images were exposed for only a small fraction of a second, by means of a shutter. As the exposure grew longer the true form of the figures came out more and more clearly. There thus seems to be a general spreading of the stimulation in all directions over the retina from the borders of the images. Then, upon a slightly longer duration of the stimulus, this very rapid irradiation of excitation contracts and the irradiation becomes confined within the borders of the stimulated portion and affects the intensity of the different portions of the image. With strong intensities and certain colors it is, however, never wholly confined to the stimulated portion even of moving images. Charpentier speaks of "clouds of light accompanying his figures." With green light these clouds are especially noticeable. His "palm branch" phenomenon is a good instance of the irradiation of stimulation.

Besides these experiments which I have just described, several phenomena of a like sort were observed in connection with other experiments which were being performed in the laboratory at the same time. Dr. Holt was experimenting with a bright circular spot of light about one half inch in diameter, surrounded by a very faint ring about one half inch wide. When the whole image was moved about, the spot would seem to go back and forth across the less intense part so that the whole image looked like a jelly-fish swimming about in the water.

When the figure was allowed to remain stationary for a few moments it would resume its natural shape. Otherwise the bright part would seem to advance faster than the rest, sometimes even overlapping the border. This phenomenon was due to the fact that a bright light requires less time in coming into consciousness than a less intense one, and is, of course, the same in principle as those which

were performed with dots when the bright dot moved ahead of the rest.

Another one of these phenomena occurred when an isosceles triangle was moved in a direction parallel to its base. The side toward which it moved appeared curved forward, with the apex bent backward. Toward the bottom, where there was the best chance for irradiation to have its effect, appeared the most advanced portion of the figure, while the bottom corner, although objectively the most advanced part of the figure, appeared rounded off and somewhat behind the part just above.

A narrow, vertical image with a large bulge behind the central part appeared with a large portion of this bulge in advance of the centre of the figure.

All these experiments show that a more intense object is, other things being equal, always located ahead of other objects co-linear with it. And I assume irradiation to account for the priority in localization of parts of the figure which are not objectively of greater intensity than others, but whose position makes them subject to reenforcement. The localization itself may be a function of more central organs, and not directly a question of the coming into consciousness more quickly of a more intense stimulation, although that seems to be the simplest explanation, but in any case priority of localization varies directly with the degree of intensity.

If the light is not bright enough to produce much irradiation the image will lose its curvature. If the light is too bright, although there may be a maximum of irradiation aroused and the absolute difference in intensity between the ends and centre of the image be at its greatest, yet this difference may not be great enough in proportion to the absolute intensity of the light to make the centre of the image appear in advance of the rest.

The curvature also varies with the angle subtended by the image and the portion of the retina upon which the image falls. If the image were too long, although all the processes which produce curvature be present, yet the front of the image would still appear vertical, because of the fact that each point in this long line would not derive reënforcement sensibly greater than that of the neighboring points. The best one could expect would be that these long figures should have their ends rounded off, which is usually the case. Most of the images which Professor Hess used in his experiments were too long to appear curved. All the images whose curvature we measured did not subtend an angle greater than 1° 10′, and were all seen on the fovea.

An image which subtends an angle of more than about 2° will hardly appear curved when it passes over the fovea.

We were sometimes able to see the curvature reversed. This happened in my own case about once in a hundred times, usually when my eyes were fatigued by the repeated passing of the moving light back and forth over the same portion of the retina. With other observers it occurred more often.

Slight vertical differences in fixation would cause the central part of the path taken by the moving light to become more fatigued than the edges and so to respond more slowly to the stimulation and reverse the curvature. It may be that some brain process which has to do with the apperception of the form and movement of visual objects becomes fatigued or does not always function properly, and so the curvature of the image may sometimes appear reversed. At any rate the more usual cases are those in which the convexity is present. The others, owing to the number of factors involved, and the vast majority of the opposite cases, may be regarded as due to temporary defects in the psycho-physical mechanism, which when properly working would give the more usual result.

Quantitative Experiments

The object of the following experiments was to measure the amount of curvature produced by differing degrees of intensity of light at different speeds. An oblong figure was employed one fourth inch wide and two inches long. As has been mentioned, two vertical wires were stretched across the path in which the light moved. As the light swung by, it was attempted to get the wires at such a distance from one another that when one appeared tangent to the curve at the front of the figure the other would seem to cross the image at the point of intersection of the curve with the rest of the figure, as indicated in the diagram. (Plate IV, Fig. 9.)

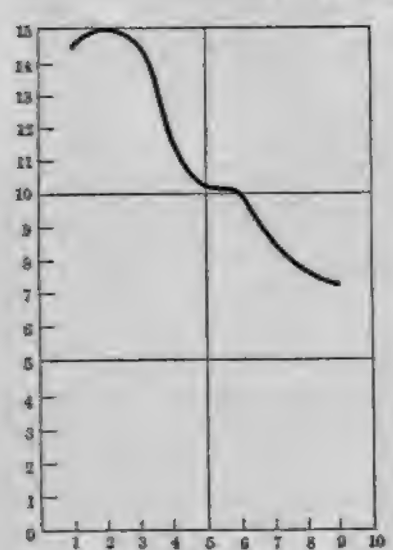
The distance between the wires was then read off on a scale. Thus one was able to obtain a measure of the curvature of the figure when it was moving at different speeds and illuminated by different intensities of light, and to compare the observations of different subjects. The mean error in this work is surprisingly little, considering the difficulties in making the judgment as the light passed rapidly by the wires. Usually the moving light had to be observed several times before the curvature of the front of the moving image could be measured exactly. It would be perfectly obvious that the front was considerably curved,

but it would often be wholly impossible to tell just how much it was curved, until the pendulum had swung back and forth four or five times. Fatigue and darkness adaptation modify the judgments considerably. If one's eyes were partially adapted to darkness some little difficulty was experienced in seeing clearly the curvature of the image. Fatigue comes on very rapidly indeed. Usually it was impossible to get more than four judgments without resting, and often only two could be made. It was sometimes impossible to measure the curvature at the exact point when the light passed under the cross-wires, so the curvature had to be observed carefully and compared with the distance between the wires, and a judgment made when the wires were not superimposed upon the image. With each intensity of light two judgments were taken, one when the cross-wires had to be brought nearer together, the other when they had to be moved farther apart. Several series of measurements were made by different observers, and the results averaged up and compared.

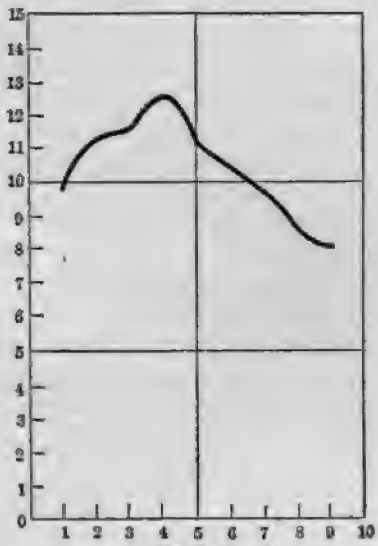
The following curves and tables give the different observations for the nine different intensities of white light,[1] and the three speeds which were used. In the case of the high speed the light moved across the opening in the screen placed before the pendulum at a velocity of about 1.5 ft. per sec. The middle speed was about 1.27 ft. per sec. and the low speed about .917 feet per sec. In all cases an oblong image was used, ¼ inch wide and 2 inches long. The numerals on the left of the plotted curves give the apparent curvature of the image in sixteenths of an inch, and were obtained by measuring the distance between the cross-wires when this distance measured the apparent curvature of the image in the way described above. The figures at the bottom designate the different intensities of light which were used. Number one is the greatest intensity, number nine the least; the others those in between.

High Speed. This curve shows very well indeed what seems to be typical of the relations between the intensity of the moving light and the apparent curvature of the front edge of the image. With the lowest degrees of intensity the amount of the curvature is very little. Sometimes it was difficult to measure it at all. The light was so faint and the speed so rapid that probably very little reënforcement or irradiation took place, although what did occur would show its presence most prominently, since, on account of the high speed at which the

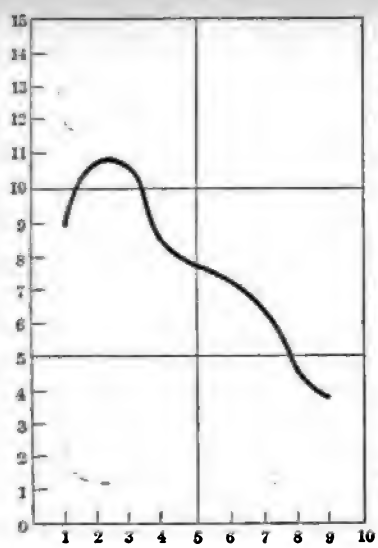
[1] The intensities used with white light are all less than an eight-candle-power electric lamp placed at about a foot behind the opening and covered with two pieces of ground glass.



High Speed



Middle Speed



Low Speed

pendulum moved, any part of the image which should come into consciousness ahead of the rest, even by a very little time, would appear considerably in advance of the rest of the image in space. Of course a certain amount of time would be required for the stimulus to spread itself over the retina, since it has to overcome a certain amount of resistance in the nerve-layers, and if this time were not given, the curvature of the resulting image would be of course decreased. As the light brightened, however, the curvature increased rapidly, until finally, when the intensity of the light neared its highest point, the curvature ceased becoming greater, and finally decreased. The mean error in eight judgments taken by two people for each intensity of light was about .099 in.

The measurements with the middle speed were very similar. The curvature with the lowest intensity of light was somewhat greater than

when this same light moved with the highest speed. The maximum point of curvature was reached with a light of less intensity, and the curvature was less. When yet higher intensities were used, the curve decreased rapidly. The amount of curvature was also much less with the brightest light than with the higher speed. The following table shows the judgments of three observers for this speed:

MIDDLE SPEED

Intensities.	1	2	3	4	5	6	7	8	9
First Subject.	8	10	9	11	10	9	9	7	8
	10	11	13	13	10	11	10	9	8
	10	10	11	11	9	8	8	7	7
	10	11	11	14	10	12	8	9	9
	10	11	10	11	9	9	7	10	7
	11	11	11	13	10	10	9	10	7
Second Subject.	10	9	11	11	12	8	11	7	8
	12	14	13	14	15	14	10	9	9
	10	14	13	13	12	11	11	10	9
	11	14	13	13	13	12	12	10	9
	13	11	13	14	12	12	10	9	9
	13	12	13	14	12	12	11	11	9
Third Subject.	7	10	9	13	10	9	9	7	8
	8	12	12	13	12	10	10	8	7
	7	11	10	10	11	10	10	8	8
	9	11	13	11	12	10	11	8	8
	9	11	11	10	10	9	10	9	7
	9	11	12	12	11	10	10	8	8
Average.	$9\frac{15}{18}$	$11\frac{1}{3}$	$11\frac{1}{2}$	$12\frac{5}{18}$	$11\frac{1}{9}$	$10\frac{1}{3}$	$9\frac{7}{8}$	$8\frac{5}{8}$	$8\frac{1}{9}$

Mean Error, .075 in.

The low-speed measurements show the same general tendencies except that the curvature is smaller with this speed when the faintest lights were used than with any of the others. The maximum curvature is also less and occurs with a more intense light than with the middle speed. These modifications offer no special difficulties. Since the light moves slowly, although the centre of the image, which is reënforced by induced excitation from the ends, does appear in consciousness in time ahead of the rest of the image, yet it does not appear so far in advance of the rest of the figure in space as it would if the light moved with a higher speed. While the same difference in brightness

between the centre and the ends of the image should make one part appear in consciousness just as far ahead of the rest in time with this speed as with any other, yet since the speed is slow it would not appear to be so far ahead in space. The fact that the maximum amount of apparent curvature is less would also be explained in the same manner.

When the high and middle speeds were used the results were surprisingly consistent and the variations between the observers not very great. With the low speed the individual differences are very much more prominent. Uncertainties and variations between observers and between different observations of the same observer became greater and greater as the intensity of the light decreased. One seemed to be approaching the lower limit of induction, below which, even if the spreading of light stimuli through the retina took place at all, it was to such a slight extent that it made no very marked difference in the appearance of the moving image. Individual differences are very great in this respect. For instance, my own average measurement was $\frac{15}{64}$ in. of curvature for the image produced when a light of the lowest intensity moved at the lowest speed. Mr. Vaughan's measurements averaged $\frac{30}{64}$ in., a measurement of just twice as much for the same light at the same speed.

So far there has been given only a general view of what happens when an oblong moving image appears convexly curved. It may be well to consider the different causes which determine a certain curvature of the image and see how they are related.

As we have seen, the curvature is a function of the difference in intensity of various excitations between the centre and the ends of the retinal area excited. This difference is modified both by the objective intensity of the light and by the speed at which the light moves. Its efficiency to produce curvature is also modified by both these factors, since it requires a certain small amount of time for the irradiation to take place. If this time is not given by the too rapid passage of the image over a certain part of the retina, the difference in intensity will be lessened and the curvature therefore be decreased. On the other hand, if the figure moves too slowly, although this difference in intensity may be as great as possible for the brightness of the light which is acting, yet the curvature may be lessened on account of the fact that, although the centre of the image does appear ahead of the ends in time, yet it does not appear so far ahead of the ends of the image in space as it would if the same difference in intensity were present and the image moved more rapidly.

It will be remembered that the intensity of the centre of the figure

owes its increase to reënforcement by excitation irradiating from the surrounding points. It seems only reasonable to suppose that this added intensity due to irradiation does not increase without limit, or with exactly the required ratio to produce a curvature of the front of the image which becomes continuously greater as the intensity of the light increases, indefinitely.

If this be so, then, at a certain brightness, the difference in intensity between the ends and the centre of the image will have reached a maximum, and a further increase in brightness of the light will not serve to increase the apparent curvature of the image, but rather to decrease it.

Color Irradiation

The color of the image has a decided influence upon the amount of perceived curvature, independently of the intensity.

The following experiments were performed with lights of different colors, in order to investigate the relations between the kinds and amounts of irradiation of the different colors by comparing the amounts of the curvatures obtained. We encountered a good deal of difficulty in fixing upon a proper method of comparison between the different colors. Finally it was decided to use such intensities of light as would give a maximum amount of curvature with each of the four primary colors, to measure this amount in each case, and also to measure the amount obtained when the intensity of the light was greater and less than that required to give the maximum.

It was found that very different objective intensities of light were required to give a maximum amount of curvature with the different colors. The colored images were obtained by placing colored pieces of glass in a frame which stood before the source of light. The intensity of the light could be regulated by interposing or taking away pieces of ground glass which rested in the frame between the light and the colored glass.

The red glass gave a nearly saturated color, but its place in the spectrum was rather nearer the orange than I could have wished. It was a thick piece of glass and absorbed a great deal of light. A 32-candle-power light with four pieces of ground glass in front of it gave a maximum curve for most observers.

The yellow gave a very well-saturated color with light from the incandescent lamps which we used. The glass was thinner and absorbed less light than the red. A 32-candle-power lamp with three pieces of ground glass usually gave the maximum.

Two 32-candle-power lamps and one of 24-candle-power were required with the green.

The green glass was not quite so saturated in color as the red or yellow. It was a slightly yellowish green. Red and yellow rays were visible through it to some extent when it was examined through the spectroscope. It absorbed somewhat less light than the red and decidedly more than the yellow. The maximum curvature was obtained when the source of light was screened with four pieces of ground glass.

The blue glass was a bluish violet, very heavy, and absorbed a great deal of light; it allowed many red and violet rays to pass through. It was necessary to use with this glass two 32-candle-power lamps and one 100-candle-power. When the combined light of these lamps was reduced by interposing three thicknesses of ground glass, the maximum curvature was observed. The light which then appeared, however, seemed of greater intensity than any other which gave a maximum.

The curvature of the white light was measured again in order to compare it with the colored lights. This was necessary, since the work was done with a different set of subjects and the former work showed individual variations. An 8-candle-power light was used as before. This, reduced by four pieces of ground glass, gave the maximum in most cases.

The following curves and tables show the average of the observations of four subjects. In the table the figures under the columns numbered 1 and 2 represent the amount of curvature perceived when the intensity of light was greater than that required to give a maximum under 4 and 5, when the light was not strong enough to produce a maximum of curvature. The columns numbered 3 represent the greatest amount of curvature perceptible with each color.

The curves shown in the diagram represent these measurements plotted out.

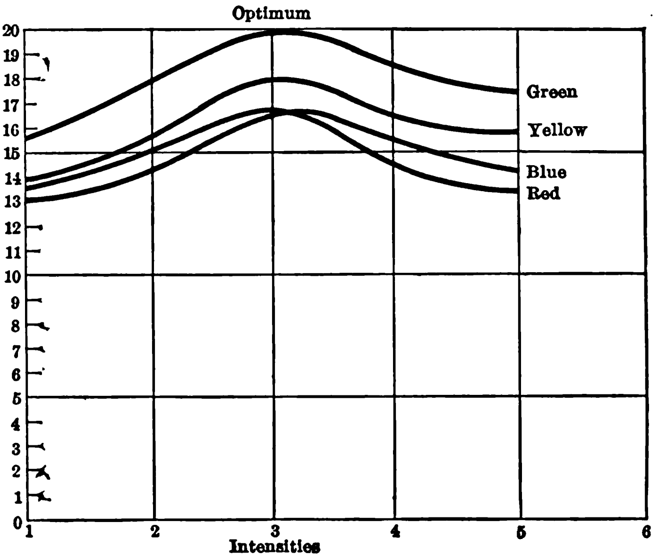
TABLE

Intensities.	1	2	3	4	5
Red.	13.50	15.20	16.85	14.06	13.46
Yellow.	13.90	15.46	18.00	16.40	15.85
Green.	15.66	18.00	19.86	18.32	18.00
Blue.	13.00	14.15	16.85	15.50	14.09
Average for all the colors.	14.00	15.70	17.90	15.90	15.35

The measurements were made in the same way as before, and are given in sixteenths of an inch.

In the diagram the abscissas represent the different intensities, the ordinates the amount of the curvature. To avoid confusion, the curve of the average of all the colors is left out of this diagram.

It will be noticed in these records that the different colors give very different measurements of curvature. Green gives by far the largest, being greater than any of the others at every point. Since the process



of obtaining the curvature was the same with all the colors, these differences in curvature can only be due to inherent differences in the processes which give the sensations of the different colors. It cannot be due simply to one sort of intensity process, the same for all the colors, otherwise the curvature of all the colors would be the same. At the same time the curvature of the image is due to differences in intensity of excitation between one part of the image and another. There must be, therefore, a retinal excitation in some respects different for each color, capable of its different degrees of intensity. Of course these individual differences would have a decidedly limited range, for, as every one knows, if the intensity of a color be increased sufficiently its saturation vanishes and white appears in its place, while if the intensity be decreased without limit, black appears. It may be that different degrees of excitation in the different processes have different rates of

time in coming into consciousness, so that an equal degree of difference in excitation between the ends and centre of the green image and the ends and centre of the red image would give decidedly different amounts of curvature, if it took a longer time after the centre of the green image had appeared in consciousness for the ends to appear than it did in the case of the red.

The time-differences might be greater with the same differences in intensities of excitation with one color and another. Or it may be that the excitation spreads in a different manner with each one of the colors, and therefore gives differing degrees of reënforcement with the different colors, and thus produces different amounts of curvature.

It is noticeable also that the amounts of curvature are related to one another in a peculiar way. Green has the greatest amount of curvature, yellow the next. Red is greater than blue with the higher intensities, they are equal at the maximum, and blue is greater than red when the lower intensities are used.

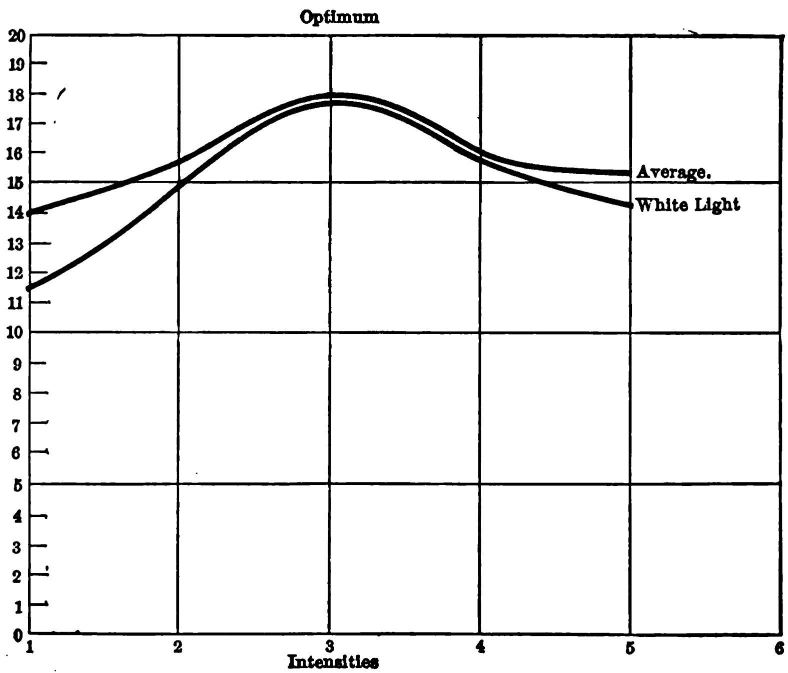
When a spectrum showing a fair degree of saturation is observed, it is seen that the point of greatest brightness lies in the yellows. As the intensity is heightened, this point moves toward the red end, and as it is lowered, it moves toward the blue.

It will be seen that the relation between the different amounts of curvature for the different colors is the same as that between the different degrees of apparent brightness when the intensity of the colors in the spectrum is decreased. It is not that of the extreme case of the phenomenon of Purkinje, but when the point of brightness has moved from the yellow into the yellowish greens or decidedly to the right of the place it occupies in the normal spectrum. In that case yellow would be the color second in brightness. In our measurements the amounts of curvature obtained from yellow images were next in size to those of green. The red and the violet-blue which we used would therefore be about equal. It is a noteworthy fact, however, that when the intensities of light (1) and (2) are too great to give a maximum of curvature the amount of curvature obtained with the red is greater than that of the blue, while with the intensities which are too small (4) and (5) to give a maximum the blue curve is greater than the red.

As yet it has not been possible for me to find an interpretation of these facts which would seem to meet all the requirements, and I should not wish to offer any explanation at present. The question of the possible connection of this phenomenon with Purkinje's is probably important for any explanation, though it is possible that the

arrangement of the curves is merely a coincidence, yet this hardly seems likely, and it would seem as if an explanation of the connection would involve an attempt at explanation of Purkinje's phenomenon, and lead at once into the most doubtful problems of the theory of visual sensations.

It is also noticed that the series of numbers obtained when the amounts of curvature of the different colored images, at the different intensities given in the foregoing table, are averaged up, and the curve of the average of all colors is thus obtained, that this average curve is very like that obtained for the white light. These curves and the series of numbers which they represent are here given.



Average for all colors.	14.00	15.70	17.90	15.90	15.35
Curve for white light.	11.50	15.00	17.80	15.75	14.25

It will be noticed, however, that the curve for the white light, while nearly equal to that of the average for the colored lights at the maximum point, nevertheless falls considerably below it at each end. This may possibly be due to the fact that with white light it was only necessary to use an 8-candle-power lamp as a source of light, so that when

pieces of ground glass were interposed in order to reduce the intensity of this light, very much greater reduction would occur with this comparatively weak source than would take place with an objective source of light of far greater brilliancy, as was the case with the colored lights. Hence there would be a greater difference in absolute intensity between intensities 1 and 3 with the white light than between intensities 1 and 3 with any of the colored lights, or that represented by their average. Thus the falling of the curve of the white light at each end may possibly be due to the fact that there is a greater difference in intensity represented by these parts of the curve in the case of the white light than is represented by the analogous portions of the curve of the average of the colored lights.

It will be remembered that these measurements were obtained when the image was upon the fovea, so that the white obtained was "cone white," and not due in any way to the functioning of the rods. It is interesting to note that the curve of the white is very near that of the average curve of all the colors, though I should hesitate to draw the conclusion from this that "cone white" is due to a mixture or fusion of all the excitations corresponding to the different colors.

In regard, however, to relations of the amounts of curvature of the images, there are several further considerations which ought to be noted. In the first place all three measurements were made when the images were entirely on the fovea. In the fovea there are no rods, so, whatever the connection of these facts with Purkinje's phenomenon, it is one which has to do with the functions of the cones alone.

Professor Hess, in his experiments upon totally color-blind subjects, found that exactly the same oscillatory processes in the course of the stimulation occurred with them as with normal subjects. He also found that the difference in the time of latent perception between the foveal and extra-foveal parts was the same for one set of subjects as for the other. The sole difference seemed to be in the one fact of not being able to perceive colors. From these facts it does not look as if the difference between seeing colors and color-blindness were by any means always due to the absence of cones in the color-blind eye. It may of course be true that an eye which is deficient in cones or which has a lesion of the fovea would have poor color perception. But it seems also true that an eye which, in so far as the rods and cones and their purely retinal processes were concerned, seems to be normal in every way, except perhaps that somewhat different intensities were required to give the same reactions (which might be explained by different central

processes), may nevertheless belong to a person who is totally color-blind or totally unable to perceive colors with that eye.

If this should prove true, the cones would still be regarded as the end organs of color perception, but the cones would only give sensations of color when functioning in conjunction with some other more central process. The usual cases of color-blindness would be attributable, not to any deficiency in the cones or any other retinal process, but to a defect in this more central process, which, working in conjunction with the cones, gives us our sensations of color.

The usual views of the functions of the rods would not be affected by these considerations. They would continue to be regarded as end organs whose main business it is to deal with weak stimulations and to notice movement in objects whose images fall upon the periphery of the retina.

But the main difference would be that all cases of partial color-blindness and most cases of total color-blindness would be explained by lesions in the brain rather than abnormalities of retinal structure.

Various Forms of Irradiation

The endeavor to explain these phenomena of moving images which we have been considering and an examination of the literature of the subject have led me to conclude that there are five distinct types of irradiation. These are:

1. Irradiation α. The very rapid spreading of the nervous excitation over the retina, which extends far beyond the borders of the image and which occurs immediately upon stimulation. It is most distinctly observed with stationary sources of illumination of the briefest duration perceptible. This kind of irradiation has been discussed at length by Charpentier and Bidwell.

2. Irradiation β. As the apparent form of the moving image becomes distinctly perceptible, such irradiation takes place within the confines of the stimulated portion of the retina, so as to make the excitation present at favorably situated localities more intense than that of other places. The portions which are so situated as to receive this reënforcement are the first to enter consciousness. The various phenomena discussed earlier in this paper furnish examples of this process, as well as the phenomenon of the curved image.

3. Irradiation γ. After, and in part during, the rise and development of the reënforcing irradiation, emanations of nervous excitation of small intensity proceed from the borders of the stimulated portions

and from the after-images, rapidly extending themselves over the retina and gradually decreasing in intensity.

4. Irradiation δ. When two fields of different intensities are brought into juxtaposition, the field having the greater intensity will enlarge itself at the expense of the other. This constitutes what has been usually termed irradiation, and is observable with stationary objects. This enlargement varies with the time during which it is observed, the absolute intensity of the light employed, and the relative differences in intensity of the two fields. Its angular extent under determinable conditions is constant, although it varies considerably from one observer to another, and with the same observer at different times. Its physiological explanation is probably similar to that of the other kinds of irradiation, viz., the spreading of nervous excitation over or through the layers of the retina, although various factors of accommodation, dispersion, achromatism, astigmatism, etc., enter in and modify the totality of the phenomenon. It will be noticed that reënforcement occurs in this kind of irradiation as well as in certain of the other forms. The sides of the dark fields upon which this form of irradiation shows itself appear curved inward at the centre, apparently showing the presence of a greater excitation in the lighter fields next to the centre of the darker ones.

5. Irradiation ε. When a luminous object has been observed for a long time (from thirty seconds to several minutes), the whole surrounding field will be flooded by a faint haze of light, which within certain limits increases in intensity the longer the stimulation is present. This phase has many characteristics of the first and most rapid kind of irradiation, and possibly represents a discontinuance of functioning, through fatigue of certain nervous mechanisms which prevent the spreading, or inhibit the perception, of this irradiatory excitation after the form of the object is distinguished clearly. It is probably largely due to such a mechanism that we are enabled to perceive as clearly and sharply as we do the outlines of objects which differ greatly in intensity from their backgrounds.

W. McDougall has developed a theory of inhibition [1] which he uses to explain the more usual kinds of irradiation. This explanation harmonizes very well with the results of my own experiments and helps to explain all the kinds of irradiation we have distinguished. Briefly stated the theory of inhibition is this: there is a transference of nervous excitation or energy through the nerves and from one neurone to another. This living nervous energy he calls neurin. The

[1] McDougall, 1901, 1903.

place where it crosses from one neurone to another he calls the synapse.

Of course these conceptions are not to be taken too literally. They seem to be rather, if they are to be of any value at all, a convenient way of handling certain neurological processes of which at present we know very little, but whose grosser modes of action are comprehended more easily by the use of such terms as "resistance," "neurin," etc. It is in this manner that I wish to be understood in the use I have made of Dr. McDougall's valuable contributions to the methodology of the subject.

Neurin is generated when a stimulus is applied to the afferent nerves. When a strong stimulus is applied, neurin is generated rapidly, and discharges across the synapse to the efferent neurone in a series of very rapid discharges like the multiple discharge of a Leyden jar. When the stimulus is weak the discharges take place more slowly. Consciousness occurs at the time of the discharges and occurs in pulses. When these pulses occur in very rapid succession we experience a continuous sensation, when the discharges take place at a lower rate we are conscious of a pulsative sensation, as for instance, in the visual phenomenon of Charpentier's bands.

Continuance of stimulation continues to produce neurin, but the multiple discharges caused by the incoming neurin cause fatigue in the synapses, and the neurin seeks new paths of discharge through unfatigued synapses.

The resistance of the synapses is first lowered by the incoming neurin, then raised again through fatigue. When the resistance is first lowered upon application of the stimulus, the neurin which might go through other channels of discharge is "drained off" through the synapses which have their resistance thus lowered, then as the resistance is again raised through fatigue, it again seeks discharge through synapses which are unfatigued.

Applying these conceptions to the different kinds of irradiation we have distinguished, we can bring them all under one category. One might remark in passing that, in so far as our purposes are concerned, it makes very little difference whether we regard consciousness as occurring upon the crossing of neurin from one neurone to another, or upon the charging and discharging of a cortical cell, so long as the conditions already referred to are maintained, viz., first, a lowering of resistance as the incoming nervous excitation finds its way through the cell or across the synapse, and then the gradual rise of resistance and its conduction into new channels by fatigue of the synapse, or exhaus-

tion of the cell and a consequent turning of the excitation through fresh cells across fresh synapses before its passage into the efferent nerves.

When a light stimulation falls upon the retina, during the first one hundredth or one fiftieth of a second the nervous excitation of neurin will spread about generally through the retina for a considerable distance from the point immediately excited. Thus by means of the fibres of the retina faint excitations will go to the brain from all these different points, so that one will perceive a faint cloud of light, similar to that described under the first kind of irradiation. Moreover, since the portion of the retina directly stimulated by the light will have the most intense stimulation, this part will come to consciousness somewhat more quickly than the outlying parts, so that the cloud of light will first seem to spread outward from its source, and then, as the resistance in the synapses is lowered through the more intense stimulation of the part of the retina upon which the light directly falls, the outrunning excitation will be "drained off" from these portions of the retina outside of the borders of the image, and the halo or cloud of light will appear to contract again. This was observed by Charpentier and Bidwell, and in our own experiments.

Moreover, in case the synapses corresponding to the portions of the retina indirectly stimulated should have themselves periods of discharge and periods of charging, we might expect to see dark rings upon this halo, this was also first observed by Charpentier and Bidwell.

Secondly, as the resistance is lowered in the central organs corresponding to the end organs of the retina upon which the stimulation falls, the image tends to assume its true form, but irradiation has been, and probably still is, present through the layers of the retina, so that certain favorably located portions of the image secure reënforcement by means of this irradiation, in the manner described, and these portions appear in consciousness sooner than the others. This reënforcement, in the case of the travelling oblong image, will make it appear convex. Moreover, since the resistance of the synapses corresponding to the centre of the oblong images will be less than those corresponding to the ends, there will be a certain tendency to "drain off" the stimulation from the rest of the image, a sort of reënforcement of the reënforcement, which will also help in making the image appear curved. Of course all the conditions which we found to modify the curvature of the images will still hold good, these conceptions being used only to describe the course of events which causes the image to appear convex. Thus a very weak or a very intense or a very long or an excessively short image will not appear curved, owing to a lack of difference in

intensity between the ends and the centre great enough to produce perceptible curvature.

As to the third kind of irradiation, that which proceeds from the ends of the moving image over the unstimulated portions of the retina, and which has the appearance of long streamers of light extending outward and backward from the moving image, this may be regarded as being in certain respects a form of the first and very rapid kind of excitation. It may well be that all the outrunning excitation which occurs immediately upon stimulation does not find its way to the central organs through those nerve-paths which correspond to the directly stimulated portions of the retina, even after the form of the image may be very clearly determined, but that some excitation proceeds outward from one retinal element to another, arousing fainter and fainter excitation as it proceeds. This being the case, we should expect to find these streamers of light from the ends of the image extending outward and backward over the retina. Of course the faster the image moved and the more intense it was, the longer then would be these streamers. For if the image moved very fast, very much less of the excitation would be "drained off" through the directly stimulated portion, and thus more of the excitation would be left behind, so to speak, by the image when it moved along rapidly, and this would appear to drag farther and farther behind. Of course these streamers being curved backward would appear more curved the faster the image moved, and if the pulsative processes occurred with these stimulations which occur in the course of other retinal stimulations, we should have Charpentier's "palm-branch" phenomenon.

The fourth kind of irradiation which we have defined is of course the best-known form, and is that which has been the most discussed by the many writers on the subject. It will be remembered that this form appears in stationary objects which have been observed for some little time (from four to ten seconds), and consists in the apparent enlargement of a more intensely illuminated portion at the expense of a less illuminated one. This enlargement occurs after all trace of the first kind of irradiation has vanished, and of course no trace of the third kind comes in, since the object is stationary. The course of events may then be somewhat as follows. In the first perception of the object we have the wide-spreading irradiation described. Then way is made through the synapses corresponding to the stimulated portion of the retina, and the wide-spreading irradiation is drained off through these open channels, so that the image contracts again to its proper

size. But at the same time it is not likely that there will not be a slight irradiatory enlargement of the borders of the image. For irradiation is present within the confines of the image. This is shown not only in the case of moving images, but also in the fact that the edges of the less intensely illuminated portions of the field are curved inward, this being most probably due to the fact that the centres of the contiguous luminous portions are reënforced by irradiation proceeding from the direction of both the ends.

Not all of the excitation proceeds to the brain from the directly stimulated portions of the image merely, but a little irradiates over the borders and causes an apparent enlargement of the brighter field. It has also been shown by Plateau and others that the amount of irradiation increases both with the intensity of the stimulation and with the time during which it acts. Of course, as to the intensity there is no question. As to the time-element, it may be that the excitation at the border spreads rather slowly outward after the previous contraction of the image to its proper dimensions, which takes place within a very short time after stimulation, until a sort of balance is reached between the tendency of the image to enlarge itself through irradiation and the tendency for this irradiatory excitation to be drained off through the nerves corresponding to the stimulated portion of the retina, after which no further apparent enlargement takes place.

In some of our experiments with dots we found that after a dot of the proper intensity of illumination had been steadily gazed at for some time the centre would appear dark. This seems to be due to the fact that the centre of such an image was reënforced by irradiation, so that the nervous mechanism corresponding to it became fatigued more quickly and the stimulation at the centre no longer gave such intense sensations as the rest of the figure, but appeared darker.

Passing to the fifth and last variety of irradiation, this seems due to fatigue in the inhibiting apparatus which reduced the spread of the first kind of irradiation. Following out the scheme we have applied, it would seem as if the channels which were first opened by the direct stimulation became blocked through fatigue, and, therefore, the excitation produced in the retina were forced to seek new paths through to the brain by means of the nerves which proceed from the unstimulated portions of the retina. Thus if the resistance through fatigue occurs slowly, the excitation which spreads may increase in intensity and in extent. So, as the resistance increased, a portion corresponding to the directly stimulated portion and its slight irradiatory enlargement of the borders would be surrounded by a cloud of light growing in size and intensity.

The measurements were made in the same way as before, and are given in sixteenths of an inch.

In the diagram the abscissas represent the different intensities, the ordinates the amount of the curvature. To avoid confusion, the curve of the average of all the colors is left out of this diagram.

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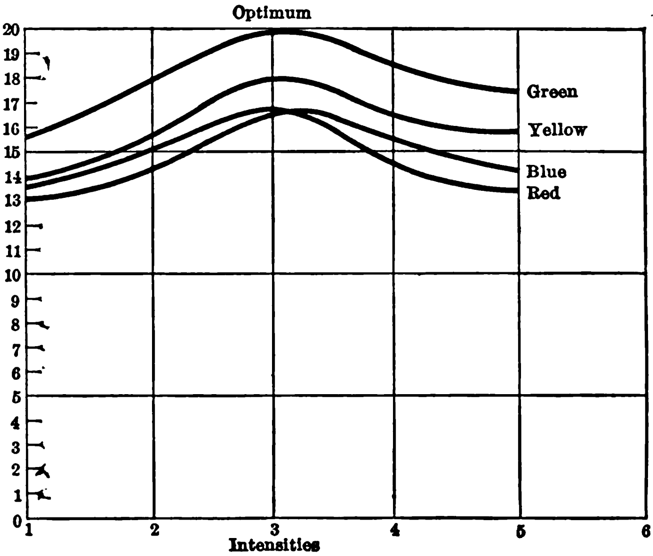


of obtaining the curvature was the same with all the colors, these differences in curvature can only be due to inherent differences in the processes which give the sensations of the different colors. It cannot be due simply to one sort of intensity process, the same for all the colors, otherwise the curvature of all the colors would be the same. At the same time the curvature of the image is due to differences in intensity of excitation between one part of the image and another. There must be, therefore, a retinal excitation in some respects different for each color, capable of its different degrees of intensity. Of course these individual differences would have a decidedly limited range, for, as every one knows, if the intensity of a color be increased sufficiently its saturation vanishes and white appears in its place, while if the intensity be decreased without limit, black appears. It may be that different degrees of excitation in the different processes have different rates of

time in coming into consciousness, so that an equal degree of difference in excitation between the ends and centre of the green image and the ends and centre of the red image would give decidedly different amounts of curvature, if it took a longer time after the centre of the green image had appeared in consciousness for the ends to appear than it did in the case of the red.

The time-differences might be greater with the same differences in intensities of excitation with one color and another. Or it may be that the excitation spreads in a different manner with each one of the colors, and therefore gives differing degrees of reënforcement with the different colors, and thus produces different amounts of curvature.

It is noticeable also that the amounts of curvature are related to one another in a peculiar way. Green has the greatest amount of curvature, yellow the next. Red is greater than blue with the higher intensities, they are equal at the maximum, and blue is greater than red when the lower intensities are used.

When a spectrum showing a fair degree of saturation is observed, it is seen that the point of greatest brightness lies in the yellows. As the intensity is heightened, this point moves toward the red end, and as it is lowered, it moves toward the blue.

It will be seen that the relation between the different amounts of curvature for the different colors is the same as that between the different degrees of apparent brightness when the intensity of the colors in the spectrum is decreased. It is not that of the extreme case of the phenomenon of Purkinje, but when the point of brightness has moved from the yellow into the yellowish greens or decidedly to the right of the place it occupies in the normal spectrum. In that case yellow would be the color second in brightness. In our measurements the amounts of curvature obtained from yellow images were next in size to those of green. The red and the violet-blue which we used would therefore be about equal. It is a noteworthy fact, however, that when the intensities of light (1) and (2) are too great to give a maximum of curvature the amount of curvature obtained with the red is greater than that of the blue, while with the intensities which are too small (4) and (5) to give a maximum the blue curve is greater than the red.

As yet it has not been possible for me to find an interpretation of these facts which would seem to meet all the requirements, and I should not wish to offer any explanation at present. The question of the possible connection of this phenomenon with Purkinje's is probably important for any explanation, though it is possible that the

FEELING

THE EXPRESSION OF FEELINGS

BY F. M. URBAN

THE material of this paper was obtained by an experimental investigation which was carried on in the Harvard laboratory from February, 1904, till June, 1905. The immediate purpose of these experiments was a study in the expression of the feeling-tone of simple sense-stimuli. Breathing and circulation were the functions the changes of which were observed by tracing the curves of thoracal and of abdominal breathing and the sphygmographic curves simultaneously. Acoustical, tactual, pain, and smell sensations were studied in this way, special attention being devoted to the smell and pain sensations. These stimuli have the advantage that the physiological reactions of the subject are more uniform than the reactions to other stimuli. The number of experiments performed in this investigation was large, although a subject was never experimented on for more than forty minutes, because the facilities of the laboratory allowed a continuous experimenting for several hours a day on different subjects. All the experiments were performed on trained subjects. Only the changes in the form of the sphygmographic curve will be discussed in this paper. The results of this observation confirm the observations of previous investigators in so far as the same changes in the curves were observed and the introspections of the subject were, on the whole, similar to those obtained by other observers. It does not seem probable, however, that a satisfactory discussion of the results can be given on the basis of merely mechanical measurements of the curves, and it, therefore, seemed necessary to reconsider the principles of the theory of the sphygmographic curves.

There are two methods which can be applied to the study of the psychology of feelings. They are called the method of impression and the method of expression. The first is a purely psychological method, while the latter is confined by its definition to the study of the physiological changes which are the accompaniments of feelings. The method of expression is never used as a pure method in investigations

which are carried on for psychological purposes, because the introspections of the subject must be compared with the physiological results. It therefore has the character of a mixed method. The first experimental investigations into the psychology of feelings were started by Fechner, who employed the pure method of impression. At this time, however, the apparatus for studying the circulation had been greatly improved and sooner or later these instruments were sure to be used for a more exact study of the influence of feelings on circulation. It was to be hoped that the crude observations on the changes of the heart-beats and of the circulation under the influence of feelings might be followed up in detail.

Darwin laid stress on the importance of certain bodily accompaniments of feelings, and he inaugurated the genetic explanation in this field. But even if the genetic explanation is successfully carried through, human psychology remains unexplained, and, furthermore, those emotional expressions which Darwin described form only a part of the physiological accompaniments which may be observed with the instruments now in use. The invention or at least the great improvement of these instruments is due to the investigators in the middle of the last century, and a more thorough understanding of the delicate changes of respiration, circulation, and of temperature was not possible before the construction of these sensitive recorders. It seems that Mosso was the first to observe these small changes under the influence of mental activity in general, and feelings in special; in this sense it may be said that Mosso started the experimental physiology of feelings. The discovery of the influence of feelings on circulation is very important, and it is to be appreciated that Mosso saw these slight changes which escaped an observer like Marey. In the Mémoire offered to the Academy [1] on March 26, 1860, Marey gives a great number of circumstances which influence the sphygmographic curve, but feelings or mental phenomena are not mentioned in this list. It is true that he speaks in a later publication [2] of the influence of "moral ideas" on the circulation and makes the hypothesis that these ideas influence the circulation in the same way as other disturbing influences, *i. e.*, by changing the peripheral resistance. At this time Marey was already

[1] Comptes Rendus, vol. 50, p. 637, 1860.

[2] Comptes Rendus, vol. 53, p. 98, 1861. "Sans rien livrer à l'hypothèse il est bien certain que des changements dans la circulation périphérique arrivent souvent sous l'influence d'émotions morales. . . . En résumé d'après ce qui précède il nous semblerait illogique de faire une exception pour les actions que les causes morales exercent sur les battements du cœur, et nous pensons qu'elles doivent agir comme toutes les autres influences, c'est à dire à la périphérie primitivement."

in possession of his sphygmograph, but nothing in this passage indicates that he saw the influence of feelings on the tracings. On the contrary, the words "Sans rien livrer à l'hypothèse" seem to indicate that Marey had no other facts in mind than those commonly known. He certainly did not follow up his observation, and his statement at this point does not differ very much from the observations of the old psychologists, that emotions change certain physiological functions, of which a more or less complete list is frequently given.[1]

It certainly is a long step from this vague statement to Mosso's experimental investigations. His new instruments, the plethysmograph, and the balance, enabled him to study the distribution of the blood,[2] and he observed the influence of mental phenomena on the circulation,[3] on the bladder,[4] and on the temperature of the brain.[5] His work, "La Paura," describes the physiological effects of emotions somewhat in detail.

The way toward applying the method of expression to the study of emotions was shown by the results of previous physiological investigations. Casual observations of the influence of certain sense-stimuli on respiration and circulation were made by Naumann, Couty and Charpentier, Thanhoffer, Dogiel, Gley, Mays, Istomanow and Tarchanoff, Féré, Delabarre, and others.[6] The changes of breathing seem to be of greater importance, and some writers account breathing the most delicate physiological index of feelings.[7] It seems, however,

[1] We quote as an instance Lotze: Medicinische Psychologie, p. 257, 1852: "Es ist wahr, dass Gefühle sehr lebhafte motorische Ruckwirkungen äussern; wir sehen die Respiration in Unordnung gerathen, den Druck der Arterienwandung auf das Blut bei heftigen Schmerzen zunehmen, Erbrechen auf widrige Geschmackseindrücke, allgemeine Muskelkrämpfe bei physischen Martern eintreten." All the principal physiological features of feelings are enumerated here, but one hardly will give any great credit for priority to Lotze. Such a general statement, in fact, belongs to the class of easy observations from which philosophical speculation often starts. G. L. Duprat shows that the same "observations" underlie the theories of Aristotle, Hippocrates, and Plato. (Duprat: La psycho-physiologie des passions dans la philosophie ancienne, Archiv f. Geschichte d. Philosophie, vol. 18 (N. F. 11), pp. 395–412, 1905.)

[2] Application de la balance à l'étude de la circulation du sang chez l'homme, Archives Italiennes de Biologie, pp. 130–143, 1884.

[3] Ueber den Kreislauf des Blutes im menschlichen Gehirn, 1880.

[4] Mosso et Pellacani: Sur les fonctions de la vessie, Archives Italiennes de Biologie, vol. 1, pp. 97–127, 1882.

[5] La température du cerveau, Arch. Ital. de Biol., vol. 22, pp. 264–311.

[6] For the literature, see P. Menz: Die Wirkung akustischer Sinnesreize auf Puls und Athmung, Phil. Stud., vol. 11, p. 61, 1895.

[7] Meumann und Zoneff: Ueber Begleiterscheinungen psychischer Vorgange

that a satisfactory treatment can be obtained only by direct comparison of the respiration and circulation, and it now but seldom occurs that circulation is observed exclusively.

There are three different instruments for observing the circulation: the plethysmograph, the sphygmomanometer, and the sphygmograph. Each of these instruments allows one to observe a different feature of the circulation. The sphygmomanometer records the pressure in the artery; the plethysmograph records the volume of a certain part of the body; and the sphygmograph records the movement of a certain part of the arterial wall. The curves traced with the sphygmograph indicate to a certain extent the pressure of the blood, and sometimes they are called curves of blood-pressure to distinguish them from the plethysmographic curves which are called curves of pulse-volume.

The invention of these instruments is due to physiological investigations of the pulse. The problem of studying the pulse by graphic, or at least experimental methods, begins with the investigations of Hales and Poiseuille. The first great success in this line was the construction of the "Kymographion" of Ludwig, but this instrument had the disadvantage that it could be applied only by scission of an artery. This circumstance, of course, confined the application of the instrument to the study of the pulse of animals. After several attempts by Hérisson, Chelius, and others, Vierordt succeeded in constructing his sphygmograph, by which curves of the normal human pulse could be obtained. Some years afterwards Marey constructed his much more sensitive instrument, which was made still handier by the use of air transmission. Buisson was the first to use air transmission for sphygmography, but Upham had used it before for similar purposes. A considerable number of sphygmographs has been constructed since, and though they may show some improvements in detail, the technique of the sphygmograph has made no marked progress since Marey, and his instrument has been found by experimental tests remarkably exact.

The curves traced with the sphygmograph are extremely variable in shape and size. In almost every normal curve, however, a steep ascent may be seen; it is called the up-stroke or percussion stroke, and this part of the sphygmographic curve has the name of the anacrotic phase. This line of ascent ends abruptly and within the limits of the usual speed of the recording drum it goes over into the descent by a sharp angle. The descending part of the curve is called the catacrotic phase. The descent is not so abrupt and is not a more or

in Athem und Puls, Phil. Stud., vol. 18, p. 3, 1901; and Wundt: Physiologische Psychologie (5th ed.), vol. 2, p. 298.

less straight line, but is interrupted by secondary elevations. The first secondary elevation is the largest and is called the dicrotic.[1]

These secondary elevations were seen first by Chelius and Vierordt, and from the beginning they aroused considerable interest. It was known that sometimes during fever the pulse takes an abnormal form, where two beats of the pulse, a strong one and a weaker one, may be felt for every heart-beat (pulsus bis feriens). This form of the pulse was thought to be entirely abnormal and it was therefore a great surprise for the first modern investigators to find these secondary elevations in tracings of the normal pulse curve. The conviction of the abnormality of the dicrotic pulse form was so firm that Vierordt always applied his instrument in such a way that it did not trace the dicrotic elevation, although it was sensitive enough to trace the exact form of the pulse curve. Marey, however, used his much more delicate instrument and found the dicrotic elevation in most of the normal pulse curves.[2] For this reason Marey's sphygmograph met at first with considerable criticism (Meissner), but the critical examinations by v. Wittich, Buisson, and Mach showed that the dicrotic elevation could not be due to an error of the instrument, for so great an error was out of question, and there no longer remained a doubt as to the genuine existence of the dicrotic elevation in the normal pulse curve. The sphygmograph, thus, had revealed two new and surprising features of the pulse; (1) The ascent and the descent do not take place with equal rapidity, the ascent being steep, the descent gradual;

[1] It has been pointed out that this terminology, which is due to a large extent to Landois, presupposes a certain theory of the origin of the secondary elevations. (Edgren: Cardiographische und sphygmographische Untersuchungen, Skandinavisches Archiv. f. Physiologie, vol. 1, p. 92, 1889.) It is not easy to change a terminology, and the Greek terms, some of which were used by and before Galen, are so indifferently connotative that they can be kept without inconvenience. If one were to be rigorous, one would change the name of the sphygmograph into palmograph, because this instrument does not serve exclusively for the registration of the abnormal pulse. It does seem, however, advisable to drop the terms recoil wave (Rückstosselevation, Landois), and "onde de rebondissement" (Marey), because they are taken from modern languages and directly suggest a certain theory with which they are intimately connected.

[2] Recherches sur l'état de la circulation d'après les caractères du pouls, Journal de Physiologie de l'Homme, vol. 3, p. 249, 1860; and La circulation du sang, p. 264, 1863. "Le dicrotisme du pouls est un phénomène physiologique, on l'observe presque chez tous les sujets; seulement il n'est sensible au doigt que dans les cas où il est extrêmement prononcé." References for previous observations of the slow descent of the pulse curve are given by Landois, Die Lehre vom Arterienpuls, p. 36, 1872.

(2) the descent is interrupted by secondary elevations. Neither of these facts could be observed by applying the finger and it seemed important to explain them. The explanation of the dicrotic promised to be of special interest, as it was shown that abnormal dicrotism is in close relation to the normal form of the pulse curve.

This caused considerable interest in the observation of the pulse, and the sphygmograph was supposed to be of the greatest importance for medical diagnosis. Burdon Sanderson,[1] Landois, Lorain,[2] Ozanam,[3] Pfungen,[4] Riegel,[5] Roy and Adami,[6] and others have studied the sphygmographic curve under abnormal conditions, and wellnigh all diseases have been studied by these observers with the sphygmograph. The results were ambiguous and did not seem to justify the amount of work spent on these observations. The enthusiasm for the sphygmograph subsided, and it was no longer expected to obtain a diagnosis, or even, indeed, a prognosis of a disease from mere inspection of a pulse curve. Later investigators, in fact, confined their research to the proof of the ambiguity of the sphygmograms, which could be valuable only in connection with other observations. It could not be hoped that an explanation of the abnormalities of the pulse curve would be found before an understanding of the normal form was attained. It, therefore, seemed necessary to decide between two theories of the origin of the normal pulse curve, which had opposed each other almost since the discovery of the existence of the dicrotic elevation. Both theories chiefly refer to the origin of the dicrotic, and they agree on this, that the dicrotic elevation is due to a wave travelling in the blood, but they disagree on the direction in which this wave is moving. These two theories may be called the theory of the peripheral, and the theory of the central origin of the dicrotic wave.

The theory of the peripheral origin of the dicrotic wave assumes that the change of pressure which is indicated by the dicrotic elevation originates somewhere at the periphery and travels through the arteries towards the heart.[7] Commonly it is assumed that the dicrotic

[1] Burdon Sanderson: Handbook of the Sphygmograph, 1867.

[2] Lorain: Études de médecine clinique; Le Pouls, 1870.

[3] Ozanam: La circulation et le pouls, 1886.

[4] Pfungen: Pulscurve der Arterien in Gad's Lexicon der medicinischen Propaedeutik, vol. 3, pp. 544–642, 1895.

[5] Riegel: Ueber die Bedeutung der Pulsuntersuchung in Volkmann's Sammlung klinischer Vortraege, Nos. 144, 145, 1878.

[6] Roy and Adami: Heartbeat and Pulsewave, The Practitioner, vol. 1, pp. 81–94, 161–177, 241–253, 347–361, 412–425, 1890.

[7] Cf. Howell: American Text-book of Physiology, p. 436, 1897. Marey's first

originates in the arterioles. This theory has been mentioned first, because it is the simpler in every respect, though the less probable. The origin of the dicrotic wave according to this theory is similar to the origin of the echo.

Buisson was the first who gave an explanation of the dicrotic elevation by assuming a central origin of this wave. His theory was adopted by Marey, who stated it in this way. The action of the heart causes the blood to be pumped into the aorta with considerable strength. The blood leaves the aorta by its inertia and expands the arterial system. In the arterioles it finds an obstacle and being reflected it flows back to the aorta. But there it finds the semilunar valves closed and a new wave is produced by reflection. This wave has an effect similar to the first, and this reflection of waves lasts until the valves are thrown open again. The existence of several secondary waves is explained by the great velocity with which the blood travels through the arterial system.[1]

This theory is open to many objections. First, there is no reason why the blood wave should not produce a dicrotic elevation when it flows back to the aorta. Second, the narrow lumen of the arterioles cannot be an obstacle to the flowing blood, because if an artery splits up into small branches, the sum of the lumina of the branches is greater than the lumen of the artery. Lack of space, therefore, cannot be the cause of the reflection of the pulse wave. Marey, finally, is mistaken in his conception of the effect of the blood pumped into the aorta by the action of the left ventricle. He supposes that the entering blood pushes before it the whole column of blood in the arteries. This view is refuted by the actual measurements of the velocity of the

explanation of the dicrotic belongs to this type (Comptes Rendus, vol. 47, p. 826, 22 Nov., 1858.) He supposed that the dicrotic elevation was due to a wave reflected from the Iliacae communes. He was led to this theory by the erroneous observation of Beau, that abnormal dicrotism never occurs in the lower extremities. Marey's own observations refuted this theory, since they show that the dicrotic elevation is found also in the sphygmograms of the arteries of the leg. (Marey, La circulation du sang, p. 274, 1863.)

[1] Marey, La circulation du sang, pp. 271, 272, 1863. "Dans ces conditions, l'ondée lancée par les ventricules se porte vers la périphérie, et par suite de la vitesse acquise, abandonne les régions initiales de l'aorte pour distendre les extrémités du système artériel. Arrêtée en ce dernier point par l'étroitesse des artères qui lui fait obstacle, elle reflue vers l'origine de l'aorte; mais cette voie est fermée par les valvules sygmoïdes. Nouvel obstacle, nouveau reflux, et par suite nouvelle ondulation (ou rebondissement). Ces oscillations alternatives se produisent jusqu'à ce qu'une nouvelle contraction du ventricule vienne y mettre fin en produisant une onde nouvelle."

pulse wave, because if it were true the pulse would appear at the same moment in every part of the body.[1]

These are the more obvious of the arguments against Marey's theory. Other investigators have tried to state a more correct theory of the central origin of the dicrotic wave. Landois's theory belongs to this type of improved theories of the central origin. The action of the left ventricle, according to Landois, causes the primary pulse wave which travels down the arterial system, until it is extinguished in the arterioles. The walls of the arteries are expanded by the arriving blood wave, and, when the valves close, they force the blood onward by their elasticity. There is a free way to the periphery, but the blood pushed towards the heart finds the semilunar valves closed and is reflected. In this way a new positive wave originates which may produce in the same way a secondary or tertiary wave.[2]

It seemed necessary first to decide between the theories of the central and of the peripheral origin of the dicrotic wave. Many investigations have been carried on for this purpose, and some of them bear witness to the high ability of the investigators. It is, however, remarkable that the arguments which have been brought forward in favor of one hypothesis chiefly consist in reasons why the other hypothesis should not be accepted. These experiments can be divided into two classes. The first class comprises all the experiments which study the relation of the pulse curve to other functions, or its dependence on various conditions. The above mentioned observations of the pathological changes of the pulse curve belong to this class. The object of frequent studies of this type has been the relation of the sphygmographic curve to the curve of the apex beat. The papers of Otto and Haas,[3] Garrod,[4] Traube,[5] Rosenstein,[6]

[1] This view was held by Haller, Bichat, and Bourgelat and goes back to Galen ("Omnes enim clare cernunt, omnes partes arteriarum eodem distendi tempore," De causis pulsi, book 2, c. 8). The first who saw that the pulse did not appear at the same time in all the parts of the body was Josias Weitbrecht, but his observations were neglected until E. H. Weber actually measured the velocity of the propagation of the pulse wave. (His famous thesis of 1827,—"Pulsum arteriarum non in omnibus arteriis simul, sed in arteriis a corde valde remotis serius

[2] Landois: Human Physiology (English translation), p. 145, 1889, and Die Lehre vom Arterienpuls, p. 188, 1872.

[3] Otto und Haas: Vierteljahrsschrift f. praktische Heilkunde, vol. 34, p. 41, 1877.

[4] Garrod: Journal of Anatomy and Physiology, vol. 5, pp. 17–27, 1870.

[5] Traube: Gesammelte Beiträge, vol. 3, p. 595, 1878.

[6] Rosenstein: Deutsches Archiv f. klinische Medicin, vol. 23, pp. 75–97, 1879.

Maurer,[1] Gibson,[2] François Frank,[3] and Edgren[4] deal with this problem. The curve of intraventricular pressure cannot be studied in man for obvious reasons, and only in some cases has an attempt been made to compare the sphygmographic curve with the curve of intraventricular pressure obtained from animals. One of the most interesting attempts in this line will be mentioned later.

To the second class belong all those investigations, by which experimental evidence in favor of one or the other hypothesis has been collected. The experiments which belong to this class are in so far more decisive as the conditions of the experiments are better known and, therefore, easier to interpret. Von Kries proved the existence of the dicrotic in the femoral artery of an animal after having replaced the heart by a bag filled with liquid.[5] Grashey[6] and Hoorweg[7] have demonstrated the existence of secondary waves in models, on which peripheral reflection was impossible. To the same type of experiments belong Marey's[8] and Grashey's registration of the waves in elastic tubes, and Mach's[9] tracings from a mechanical model on which the resulting movement of two simple components could be registered. Without giving any physiological theory Mach showed how curves similar to the pulse curves can be obtained by the registration of a movement, the mechanical conditions of which are known.

As the results of these investigations, we may state the following facts as arguments against any hypothesis of the peripheral origin of the dicrotic elevation.

(1) Automatic registration of the pulse wave shows that the dicrotic

quam in corde et in arteriis cordi vicinis fieri.") For the results of other measurements see Tigerstedt: Physiologie des Kreislaufes, p. 385, 1894. Some use of these measurements is made in the present writer's L'Analyse des Sphygmogrammes, which is to appear in the Journal de Physiologie et de Pathologie Générale for May, 1906.

[1] Maurer: Deutsches Archiv f. klinische Medicin, vol. 24, pp. 291–341.

[2] Gibson: Journal of Anatomy and Physiology, vol. 14, pp. 234–240, 1879.

[3] Fr. Frank: Travaux du laboratoire Marey, pp. 301–327, 1877.

[4] I. G. Edgren: Skandinavisches Archiv f. Physiologie, vol. 1, pp. 67–152, 1889.

[5] v. Kries: Studien zur Pulslehre, p. 62, and M. v. Frey, Die Untersuchung des Pulses, p. 164.

[6] Grashey: Die Wellenbewegung elastischer Röhren, p. 166, 1881.

[7] Hoorweg: Archiv f. d. ges. Physiologie, vol. 46, p. 143, 1890.

[8] Marey, *loc. cit.*, pp. 267–271, and Traité de Physique Biologique (publié par d'Arsonval, Chauveau, Gariel, Marey), vol. 1, p. 390, 1901; these tracings are reproduced rather frequently; *e. g.*, Pfungen, *loc. cit.*, p. 563, and Chapman: Human Physiology (2d ed.), p. 270, 1899.

[9] E. Mach: Sitzungsberichte der K. Akademie der Wissenschaften, vol. 47 (2), p. 43, 1863; and in the tables, figs. 48–53.

appears sooner in the regions nearer to the heart than in regions which are more distant. The opposite would be the case if the dicrotic elevation were due to a wave travelling from the periphery to the heart.

(2) The dicrotic appears at the same time after the primary wave in a dwarf as in a tall man. This would be impossible if the wave had to travel so much farther.

(3) Inhalation of amyl nitrite makes the dicrotic almost disappear. The adherents of the theory of the peripheral origin of the dicrotic wave explain this fact by supposing that this drug dilates the arterioles and makes little reflection possible. Their opponents say that the action of the heart and the resistance of the system are so enfeebled that the backward flow is slight and gives rise only to a small wave.

(4) If an artery is opened and the blood allowed to spurt on a revolving drum of white paper a curve is obtained which shows the dicrotic elevation (the hemautographic curve of Landois). The resistance of the periphery is totally lacking in this case and the dicrotic elevation could not appear if it were due to a wave reflected at the periphery.

(5) The appearance of the dicrotic is not retarded if an elastic tube is placed between the periphery and the place where the instrument is adjusted. If the dicrotic were due to a wave reflected at the periphery it would be retarded because the wave would have to travel a distance so much greater.

These arguments prove the impossibility of the theory of the peripheral origin of the dicrotic wave. Also the other hypothesis meets with a number of serious difficulties, and we mention the following facts which are arguments not against any special form of this theory, but against any hypothesis which starts from the assumption that the dicrotic elevation is due to a wave travelling from the heart to the periphery.

(1) The descent of the catacrotic phase ought to be a succession of diminishing waves, but not a slow descent with merely small elevations.

(2) This hypothesis accounts for none of the abnormal pulse forms.

(3) The blood ought to push against the semilunar valves with a force not less than $\frac{1}{2}$–$\frac{2}{3}$ of the force of the contraction of the ventricle, because this is about the relative height of the first secondary elevation with regard to the primary wave, which is due to the contraction of the ventricle.

(4) It does not account for the disappearance of the dicrotic

elevation through lack of elasticity of the arterial wall: for the dicrotic elevation is most marked in youth, becomes lower in old age, and disappears in diseases like atheroma and arteriosclerosis, which impair the elasticity of the arterial wall. Landois's theory overcomes this theory only apparently: although the dicrotic would be absent, yet in that case the descent of the primary wave ought to be as steep as its ascent.

(5) This theory is refuted by the experiment of v. Kries, who proved the existence of the dicrotic if the heart is replaced by a valveless bag.

The obvious impossibility of making the theories agree with the facts does not permit one to accept any of them. All of them are based on the supposition that the dicrotic elevation is due to a wave travelling in the blood, and this belief is founded on the following argument: If a wave travels in the blood the sphygmographic curve shows an elevation; the dicrotic elevation is an elevation in the sphygmographic curve. Therefore, the dicrotic elevation is due to a wave travelling in the blood. This fallacy is responsible for the astonishing fact that the refutation of one of two apparently contradictory statements does not prove the other. It is characteristic of the present state of the problem concerning the origin of the dicrotic elevation, that a modern writer [1] calls it "inextricably complicated."

The contradiction between the theories of the peripheral and of the central origin of the dicrotic, however, is only apparent, and neither may be true, because it might be that this elevation is not due to a wave which travels in the blood. The experiments of the previous investigators seem to point in this direction. The disappearance of the secondary elevations when the arterial wall has lost the properties of an elastic body, the above-mentioned experiments of v. Kries, and the observations of Grashey and Marey on the movements of the walls of an elastic tube indicate clearly that nothing but elasticity is needed to produce these secondary or dicrotic elevations, for, in the different experiments, they are produced as well when the heart and its valves are replaced by a valveless bag as when the function of the valves is unimpaired; as well with resistance at the periphery as without, the only condition being that the walls are elastic. This proves the im-

[1] L. Hill, in Schaefer's Text-book of Physiology, vol. 2, p. 111, 1902. The same opinion may be found in Hermann's Lehrbuch der Physiologie (12th ed.), p. 79, 1900. "Ihre (der dicrotischen Wellen) Erklärung ist noch nicht widerspruchsfrei gestellt." In the previous editions different views were given, and this critical doubt may be regarded as the final outcome of the investigations of almost half a century.

portance of the elasticity of the arterial wall. The experiments of the graphic registration of the movements of the walls of an elastic tube, furthermore, indicate that the conditions of this experiment are a close imitation of the mechanical conditions which prevail in the arteries. It may be expected that the analysis of the conditions of the experiment will give an insight into the origin of the sphygmographic curves, because the tracings which Grashey and Marey took from the walls of a rubber tube resemble closely the tracings of the human pulse. This experiment, first, proves that the form of the curve depends merely on physical conditions. The movement of a point of the wall of the tube depends on the following four factors: (1) The elasticity of the wall; (2) the incompressibility of the liquid; (3) the form of the original wave, *i. e.*, the way in which the liquid is pumped into the tube; (4) the rate of outflow. If the process of pressing liquid into the tube is repeated regularly, a stationary form of movement will be obtained eventually; the amount of outflow for one interval is constant in this case. This means that eventually a state is attained where the same quantity of liquid which is pumped into the tube at one end flows out from the tube at the other. The physiological bearing of this result is that the turgor of an artery does not change without a cause. Such a change would be indicated by the going up or down of the base-line of the tracing.

The first two factors are, in physiology, studied with relative ease. The elastic qualities of the arteries have been studied since Poiseuille and John Hunter by Wertheim, Zwardemaaker, Marey, and others, and they are more or less well known. The physical properties of the blood are very nearly those of an incompressible liquid, and this is certainly true for the small pressure to which the blood is exposed in the arteries.

As to the initial form of the wave which the action of the left ventricle produces in the arterial system, we get a hint from the experiments of Grashey, v. Kries, and Marey, where the sudden compression of a bag furnished the initial shock.[1] These changes of pressure can be represented by a curve like that in Fig. 1.

So long as the contraction of the left ventricle lasts and the valves are open, the action of the heart produces a certain pressure in

[1] Sudden compression is the most convenient way of producing a wave in a liquid which is enclosed in an elastic tube. It was used already in the first experiments on the propagation of these waves. (E. H. Weber: Anwendung der Wellenlehre auf die Lehre vom Kreislaufe des Blutes und insbesondere auf die Pulslehre, Berichte d. Kgl. Sächsischen Ges. d. Wissenschaften, Math.-Phys. Cl., p. 177, 1850.)

the aorta, but the influence of the intraventricular pressure is zero when the valves are closed. The second phase of the curve Fig. 1, where the pressure is zero, certainly gives the influence of the intraventricular pressure during the diastole, because there is no communication between the ventricle and the arterial system when the valves are closed. The question is whether the rest of the curve can represent the changes of the intraventricular pressure when the valves are open.

Fig. 1. Changes of pressure produced in a bag by sudden compression.

Fig. 2. Decreasing amount of liquid in a tube when the outflow is uniform.

The first curves of intraventricular pressure were traced by Chauveau and Marey. These experiments were made on a horse, and they have been repeated since it was discovered that they can be performed also on smaller animals. Besides Chauveau and Marey may be mentioned the names of Fick, Huerthle, v. Frey, Rolleston, Bayliss and Starling. The curves obtained by various observers belong to two types; one shows the so called "plateau," the other does not. Recent experiments have proved that this difference of results is due to a difference in methods. This also is suggested by the fact that different curves have been obtained from animals of the same species. Two methods have been applied lately for testing these curves of intraventricular pressure. The first was devised by Bayliss and Starling. It consisted chiefly in the photographic registration of the movement of the liquid in a manometer tube. The photographic registration is frictionless, and the mass of the moving liquid was so small that vibrations by inertia were fairly excluded for pressures which are not greater than the intraventricular pressure.[1] The second method was used by Porter. The idea of this method was to trace only a part of the curve, not the whole. The writing lever, thus, has in the beginning of the tracing no inertia at all, and the tracing may be overdrawn but is certainly correct in form up to the next point of inflexion of the

[1] Bayliss and Starling: On the form of the intraventricular and aortic pressure curves obtained by a new method, Intern. Monatsschrift f. Anatomie u. Physiologie, vol. 11, pp. 426–435, 1894.

curve.[1] These tests and the repeated experiments of Chauveau leave no doubt as to the existence of the plateau.

The varying pressure from the heart which produces the pulse wave may be described in this way: The pressure suddenly rises to a maximum and maintains it for a certain time; when the semilunar valves close, the pressure drops as suddenly as it rose, and remains at zero until the valves open again. Such a function can be represented by a curve like Fig. 1, and this is the reason why the complicated action of the heart can be superseded by the compression of a bag without changing the mechanical conditions of the problem. Of course it can not be expected that a schematic curve will show all the details of the real tracing. It is suggested, however, by Frank [2] that many of the small irregularities of the curve of intraventricular pressure are due to vibrations caused by the inertia of the apparatus and that the true form of the curve of intraventricular pressure is very simple. This remark is supported by Huerthle,[3] who tested the apparatus of Marey, Knoll and Grunmach. Marey's tambour was found to be the most exact, but even this instrument produces deformities in the tracings, though the general outlines are exact. This would indicate that the schematic representation of Fig. 1 is a very close imitation of the real form of the curve of intraventricular pressure, although empirical tracings do not show right angles and straight lines. It seems, however, that the undulations of the plateau are genuine, since they are found in the most reliable tracings, and it may be possible to explain them merely on the basis of the physical conditions of the experiment.

The fourth factor of importance is the rate of outflow. We may introduce the following assumption as to the rate of outflow of the blood through the capillaries: The outflow through the capillaries is uniform in the short time of one heart-beat. The fact has been mentioned above that the quantity of outflowing blood must be equal to the quantity of incoming, for any stationary form of the pulse movement; this new hypothesis means that the velocity of the outflow is constant. One might think that this assumption is warranted by the law of Poiseuille that the amount of outflow through a hori-

[1] W. T. Porter: A new method for the study of the intraventricular pressure curve, Journal of Experimental Medicine, vol. 1, pp. 296–303, 1896. A similar method was used by O. Frank: Ein experimentelles Hülfsmittel für die Kritik der Kammerdruckkurven, Zeitschrift f. Biologie, vol. 35, pp. 478–480, 1897.

[2] O. Frank, *loc. cit.*, p. 480.

[3] Huerthle: Beiträge zur Hämodynamik, VIII, Zur Kritik des Lufttransmissionsverfahrens, Arch. f. d. ges. Physiologie, vol 53, pp. 281–331, 1892.

zontal capillary filled with liquid under constant pressure depends on the fourth power of the radius and on the difference of pressure at the two ends of the tube, and is inversely proportional to the constant of friction and to the length of the tube. This law has been proved mathematically and tested physically only for horizontal tubes and constant pressure. Neither of these suppositions holds for the capillaries of the arterial system. The connection between the hypothesis in question and Poiseuille's law is this. Let us suppose that an artery splits up in a great number of arterioles which go off in every direction. The amount of outflow is then a complicated function, because the law of Poiseuille does not hold for every direction of the capillaries; but it will be equal to the outflow through a tube of certain radius and certain direction in the same time. Our assumption says that the law of Poiseuille holds for this typical but imaginary tube. The essential point of this hypothesis is merely the supposition that the outflow of blood through the capillaries follows *a* law.[1]

It is possible to show that the graphic registration of a movement under these four conditions must give curves which correspond to the pulse curves in every respect. The action of the left ventricle causes the pulse wave which travels through the arterial system with considerable velocity. This wave expands the arteries and the whole system is filled with blood because the wave arrives by its great velocity at the periphery before the contraction of the ventricle is finished. The increased pressure forces the blood to enter the arterioles, through which it passes at a constant rate. When the valves are closed, the amount of blood decreases uniformly and the volume of the blood contained in an artery can be represented graphically by a straight line of more or less steep descent, as is shown in Fig. 2. Now the walls of an artery have to a high degree the qualities of an elastic body, and, therefore, they are forced back by elasticity after being displaced from the position of equilibrium by the shock of the arriving pulse wave. The movement of a point of the arterial wall, therefore, results from two components: (1) From the movement which it would

[1] The special assumption on the rate of outflow is by no means essential for the following theory. Two other possible assumptions are mentioned in the author's "L'Analyse des Sphygmogrammes," and others may be found easily. Every one of these theories is equally probable as long as no experimental evidence can be brought forward. The assumption that the rate of outflow through the arterioles is uniform has the merit that it is the simplest and that it can be deduced from considerations of the average directions of tubes which split up in "every" direction.

perform if it were merely forced to remain on the surface of the blood in the artery, and (2) from the movement due to the elasticity of the arterial wall. Both movements have the same direction, because the column of blood is enclosed in a cylinder the radius of which decreases regularly, and the elastic force of the arterial wall is directed towards the centre. The direction of both forces is in the line of the radius, and the resulting movement of these two components, therefore, can be found by simple superposition. Of the first component we know that it can be represented graphically by a straight line.

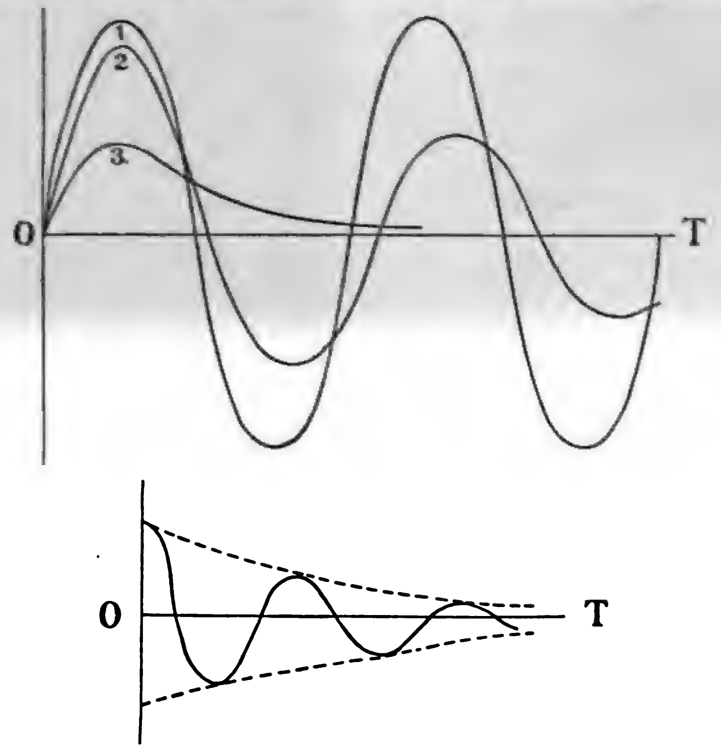
An elastic force tends always to bring the body back to the position of equilibrium; if the distance is not too great, the force is proportional to the elongation. A physical body is always under the influence of friction, the acceleration of which is opposite to the direction of the movement, and therefore diminishes the velocity. The form of the resulting movement depends on the amount of friction, and, roughly speaking, we may distinguish two types of elastic movements:[1] the first type is a periodic movement, the second an aperiodic. Let us suppose that a body is carried from its position of equilibrium by a sudden impulse, which transmits a certain velocity to the body. Friction and elasticity diminish this velocity, and after a certain time the body attains a maximum elongation, where the velocity is zero. Then the body returns under the influence of elasticity and under the retardation of friction. There are two cases possible, either the elastic force is strong enough to overcome friction and to carry the body over the position of equilibrium, or it is not strong enough. In the first case, it is easy to see, the body repeats the same form of movement on the other side of the position of equilibrium, and the conditions being constant a vibratory movement results as the stationary form. In the second case the body approaches the position of equilibrium asymptotically. The first case may be illustrated by the vibrations of a magnet needle suspended with little friction, the second by the movement of a door which is regulated by a well-working shutter.

These forms of the movement of a body under the influence of elasticity and friction are illustrated in Fig. 3.

Curve 1 shows a movement where friction is so small that it can be neglected; it is, of course, a simple sine curve. Curve 2 shows the effect of friction on vibrations. The period of damped vibrations is greater

[1] A detailed discussion shows that four different cases are possible, but this distinction is of minor importance for the purpose of this paper. The distinction holds that the movement is either periodic or aperiodic.

than in the frictionless movement, but the amplitudes are smaller. The amplitudes of a damped vibration decrease constantly and there is a simple relation between two subsequent amplitudes. The ratio between them is constant, and, therefore, if one amplitude and this constant ratio are known, all the other amplitudes can be calculated. The amplitudes of such a movement decrease as the terms of a geometric



Figs. 3 and 4

series. The dotted line in Fig. 4 represents the rapidity of this decrease. It is obvious that the smaller the constant ratio of two subsequent terms is, the more rapidly will the amplitudes decrease. This ratio depends on friction, and becomes smaller when friction becomes greater. A vibration under heavy friction dies out quickly. Curve 3 shows a movement where friction is too great to allow any vibrations. The body does not acquire a velocity which can carry it over the position of equilibrium, but it approaches this position with ever diminishing velocity.

These are the types of movement which the arterial wall can per-

form by its elasticity in consequence of the shock of the arriving pulse wave. The mechanical nature of the components on which depends the form of the sphygmographic curve is, therefore, known. The constructions in Fig. 5 show how the resulting movement can be found.

These curves are constructed in this way. The lines *AB* represent the time of the interval of one heart-beat. The straight line *EB* represents the decreasing volume of the artery and the curves on *AB*

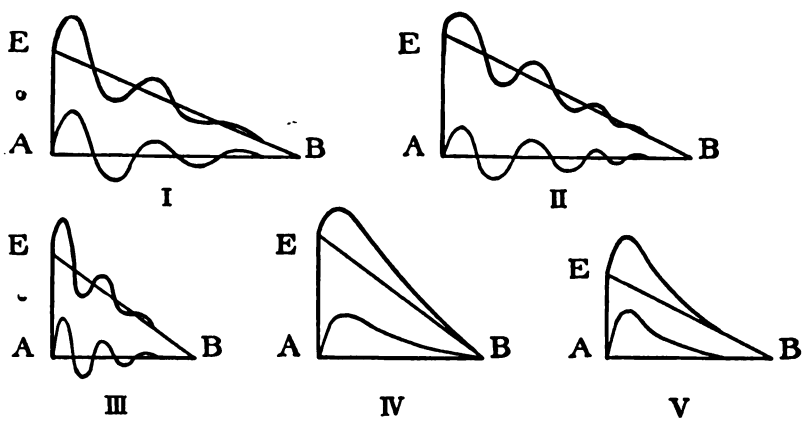


Fig. 5

represent the elastic movement of the arterial wall. Both are synchronous movements, and a line perpendicular to *AB* gives the corresponding points. The points of the resulting movement are found by arithmetical addition of the two ordinates. The results of these constructions prove that the curves show the dicrotic elevation only if the elastic force is great enough to make a vibratory movement possible. Aperiodic movements do not produce this elevation. The friction is always great for the movement of the walls of an artery, and there are only the two possibilities, of a vibratory movement which dies out quickly, and of an aperiodic movement. This accounts for the fact that the dicrotic elevation may be missing sometimes, and that in other cases several secondary elevations may be seen, the number of which, however, is always limited, and their relative height rapidly diminishing. It may be remarked that the length of the lines *AB* seems essential to the form of the resulting curve. Curves I and III differ very much in the length of the lines *AB*, while the lines *AE* are equal and the vibratory movements are only slightly different. The resultants, nevertheless, seem to differ very much. It is easy to see that a different speed of the recording drum will have an effect on the

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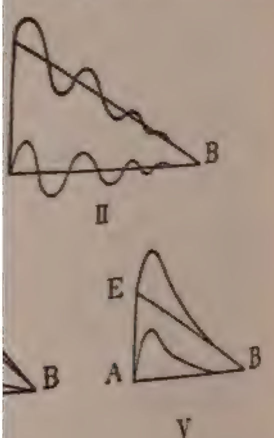

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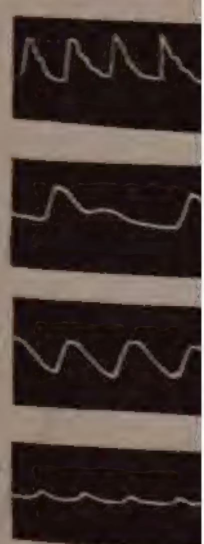

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the pulse curve shows nothing but the slight elevation of the travelling wave; No. IV in Fig. 6 shows a curve of this character.

This theory explains many surprising facts which resisted every attempt at explanation. The anacrotic part shows a steep ascent, because it is due to the sudden arrival of the blood wave. It seems that an interruption in the descent may be seen only in abnormal cases. The sphygmograms of twelve normal individuals were observed regularly by me during more than a year without once discovering an anacrotic elevation.

The hemautographic curve of Landois is produced in this way. The form of this curve depends on the velocity of the escaping jet of blood. The velocity of the blood flow depends on the resistance of the arterial system in the sense that the velocity decreases when the resistance increases. When the arterial wall is in the negative phase of vibration the lumen of the artery is smaller, and, therefore, the velocity smaller. This is confirmed by the actual tracings of the velocity of the circulation by Marey.

It is also obvious that the dicrotic elevation never can arrive before the primary wave, because the arterial wall cannot perform elastic vibrations before it is expanded by the impulse of the arriving blood wave. Neither is it surprising that the "dicrotic wave" seems to travel in the same direction and with a velocity equal or almost equal to the velocity of the pulse wave. Such a difference can be produced only by a difference in the time of the vibrations of the arteries at different points of the body. The time of one vibration is necessarily very short, and the length of this interval depends on the circumstances which determine the elasticity of the arterial wall and the friction. These conditions may be subjected to local variations. If, therefore, the time-interval between the primary and the secondary elevation is measured at two different points (*e. g.*, at the carotid and at the radialis) a difference of time may be found. Starting from the supposition that the dicrotic elevation is due to a wave travelling in the blood, one could attribute this difference of time to a velocity of the "dicrotic wave" which is slightly different from the velocity of the primary wave. The fact that the dicrotic elevation appears later in places farther from the heart was interpreted as a proof that the wave travelled out from the heart. No theory which assumes that the dicrotic elevation is due to a wave travelling in the blood can give a reason why two waves of the same form and origin should travel through the same liquid at different velocities.

At this point a theory must be mentioned, which was brought for-

ward recently, because it is based on measurements of the velocity of propagation of the dicrotic wave. This theory is connected with Krehl's theory of the function of the valves. The blood, according to Krehl, enters the aorta through a small opening, and expanding in a large space it produces fluctuations and eddies, which would close the valves if they were not kept open by the blood which streams through under high pressure. They must, therefore, close at the moment when the aortic pressure is equal to the intraventricular pressure. This occurs shortly after the moment indicated by the beginning of the decline of the intraventricular pressure curve. Now the second sound of the heart is heard somewhere in the descending part of the cardiogram [1] and the measurements of Huerthle [2] have shown that the second sound is heard 0.02″ after the beginning of the descent of the cardiogram. This seems to indicate that the second sound of the heart is in a temporal relation to the closure of the valves. Many theories of the origin of the sounds of the heart agree on this one point that the second sound is due to a noise in the muscles. It therefore may be supposed that the second sound is due to the tension of the valves when they close or shortly afterwards. The problem now would seem to be to find an elevation in the descending branch of the curve of intraventricular pressure, or in the tracings of the apex beat, which could be attributed to the closure of the valves. It was taken for granted that the curves of intraventricular pressure and those of the apex beat were identical. In many of these tracings an elevation was found which may be called "the wave *f*." This elevation is not found in all the tracings, and its position seems to be rather variable. Edgren [3] remarks that the wave *f* was always found near the abscissa no matter whether the preceding decline of the curve was great or small. In some of Chauveau's tracings the wave *f* is missing or indistinct,[4] in others it is very well marked and approximately in the middle of the descending branch of the curve.[5]

[1] Edgren: Kardiographische und sphygmographische Untersuchungen, Skandinavisches Archiv f. Physiologie, vol. 1, pp. 88–91, 1889; Fredericq: Vergleich der Stoss und Druckcurven der rechten Herzkammer des Hundes, Centralblatt f. Physiologie, vol. 7, p. 770, 1893; Einthoven und Geluk: Die Registrierung der Herztöne, Archiv f. d. Ges. Physiologie, vol. 57, p. 631, 1894.

[2] K. Huerthle: Beiträge zur Hämodynamik, Archiv f. d. Ges. Physiologie, vol. 60, p. 281, 1895.

[3] Edgren: *loc. cit.* p. 87.

[4] A. Chauveau: Inscription électrique des mouvements valvulaires, Journal de Physiologie et de Pathologie Générale, vol. 1, p. 388, fig. 4, 1899.

[5] *Ibid.* p. 391, fig. 6 (curve 5); and the same author's La pulsation cardiaque, in the same Journal, vol. 1, p. 795, fig. 5, and p. 796, 1899.

Edgren made experiments on the temporal relation of the wave *f* and of the dicrotic wave, which to avoid misunderstandings he calls the "wave *f'*." His experiments were made as follows. A sphygmogram from the carotid and a cardiogram were taken simultaneously, the points of the writing-levers being in the same vertical line. The wave *f'* appeared a little after the wave *f*. The length of this interval could be calculated by measuring the distance between these waves, as the speed of the drum was known. From this was subtracted the time of propagation of the dicrotic from the heart to the point where the instrument was fixed. In this way it was found that the time between the appearance of the wave *f* and of the wave *f'* was equal to the time of propagation of the dicrotic wave from the heart. Edgren concluded that the dicrotic wave is in close temporal relation to the closure of the valves.[1] To this comes the supposition that the wave *f'* is due to a change of pressure proceeding from the heart. The wave *f'*, therefore, could be attributed to the tension of the valves.[2] Edgren and Tigerstedt are the chief exponents of this theory.

In so far as this theory assumes that the dicrotic elevation is due to a wave travelling from the heart to the periphery,[3] it is open to all the arguments against a theory of the central origin of the dicrotic wave. Against the more special assertion that the dicrotic elevation is in connection with the closure of the valves, the following facts must be mentioned. We grant that the tracings of the apex beat may be directly substituted for the curves of intraventricular pressure, although this is by no means obvious, since one tracing gives the form of the pressure changes and the other the effect of the shock of the heart against the wall of the chest. It is, furthermore, not proved that the wave *f* is due to the closure of the valves and that the waves *f* and *f'* correspond to each other so closely as Edgren's experiments seem to indicate. His measurements of the length of lines were made with an exactitude of 0.1 mm., but his computations were carried to the third decimal place of a second. The third decimal is generally inexact and the second in a large number of cases. Experimental evidence, furthermore, directly contradicts the statement that the

[1] Edgren: *loc. cit.* p. 114.

[2] The exposition of this theory may be found in R. Tigerstedt: Intracardialer Druck und Herzstoss, Ergebnisse der Physiologie, vol. 1, pp. 258–262, 1902. This theory, equally remarkable for its logical beauty and for its confirmation by Edgren's experiments, has not found its way into recently published text-books of physiology, though Edgren's paper belongs to the most frequently quoted publications on sphygmography and cardiography.

[3] Tigerstedt: *loc. cit.* p. 261.

dicrotic elevation corresponds to the wave *f*. Fredericq[1] traced pressure curves in the ventricle and in the aorta, and determined the points of equal pressure in both curves. He thus found that a point near the beginning of the descent of the curve of intraventricular pressure corresponds to the dicrotic. His experiments are rather conclusive against the theory in question, since the wave *f* is very well marked in these tracings of Fredericq. The following facts, however, are fatal for the theory that the closure of the valves causes the dicrotic elevation: The dicrotic wave disappears in diseases like atheroma and arteriosclerosis which do not impair the function of the valves, but affect the elasticity of the arterial wall, and it is not affected by valvular insufficiency. The independence of the dicrotic from the function of the valves is conclusively proved by v. Kries, who found the dicrotic elevation in the femoral artery of an animal whose heart was replaced by a valveless bag.

All these facts, on the contrary, can be understood easily in the light of the theory that the sphygmographic curve gives the movements of the arterial wall, which movement is conditioned by the decreasing amount of blood in the artery, and the elastic vibrations of the wall around a variable position of equilibrium. In some cases the conditions of the problem are rather simple, and admit an analytic treatment, the results of which fit closely to the experimental facts. This part of the theory, however, has merely physiological interest, and therefore is discussed in a separate paper. It may be mentioned at this point that this theory of normal dicrotism is essentially identical with the theory of abnormal dicrotism as stated by Galen. He believed that the second beat of the pulsus bis feriens was due to an elastic vibration of the arterial wall. "Ex eodem genere sunt dicroti; nam arteria in occursu quasi repellitur, moxque redit. . . . Neque enim tum arteria contrahitur, sed quasi concuteretur, occidit; cuius delapsum a primae distentionis termino nulla dirimit manifesta quies, ut animadvertitur in contractione: sed simulatque attolli destitit, recidit atque ita paulisper vibrata, mox occurrit iterum."[2] Galen, however, is mistaken in his view, and in his observation that sometimes three or more pulse-beats may be felt with the finger. No form of the pulse is known where three or more beats may be felt for every heart-beat, and the actual tracings exclude the possibility

[1] Fredericq: La pulsation du cœur chez le chien, no. 5. La comparaison du tracé du choc du cœur avec celui de la pression intraventriculaire, Travaux du Laboratoire de Liège, vol. 5, p. 67, 1896.

[2] Galenus: De pulsuum differentiis, lib. I. c. 16.

of this observation for the pulsus bis feriens. The pulsus bis feriens is due to an increase of the frequency of the heart-beats. If the new pulse wave arrives before the vibrations of the arterial wall have had time to subside, the new wave and the already existing vibration may interfere in such a way as to produce this abnormal pulse form.

The form of a single wave of the sphygmographic curve may be influenced by changes in the following conditions:

(1) The pulse wave may have an initial form which cannot be represented by the schematic curve in Fig. 1. This may be due to an irregularity of the function of the ventricle. The action of the heart has an influence on the length of the waves, which length is determined by the rapidity of the heart-beats. This influence has been mentioned before. A change in the rapidity of the heart-beats has no great influence on the form of the catacrotic part of the curve so long as the impact of the new pulse wave does not arrive before the vibrations of the arterial wall have had time to subside.

(2) Differences of the elasticity of the arterial wall affect materially the form of the catacrotic part of the sphygmographic curve. It has also some influence on the height of the curves, because the amplitude of elastic vibrations depends on the elastic force for a given force of the shock. The degree of elasticity of the arterial wall is subjected to individual variations, and it depends in a given subject on the state of innervation of the wall.

(3) The surrounding tissues have a certain influence, since their resistance determines the friction opposing the vibration. This accounts for the fact that merely local conditions, such as a change of the position of the arm or the adjustment of the instrument, may change the form of the pulse curve. For instance, if the sphygmogram is taken from the a. radialis the instrument is placed between the styloid process of the radius and the tendon of the flexor carpi radialis. In the neighborhood of this place are two venae comites and a superficial branch of the median or radial vein. A change in the position of the arm will have a certain influence on the circulation in the veins, and influence the turgor of these vessels. Increased turgor increases the friction, and thus produces the different forms of the tracings.

(4) The changes of the turgor of the artery, moreover, cause a general rise or lowering of the curve. This symptom is essentially ambiguous for the turgor of the artery may be changed as well by an increase or decrease of the amount of blood pumped into the arterial system as by a decrease or increase of the amount of blood which passes through the capillaries.

The influences mentioned under (1) may be seen in tracings taken from cases of cardiac insufficiency, and have merely pathological interest. All the other influences, however, can be observed in the curves which are traced for psychological purposes. The changes in the general rise or fall of the curves are not so very hard to observe,[1] and for the observation of the rapidity of the heart-beats it is only necessary to trace a time-curve and count the number of beats or measure the length of every single beat. Also the changes of the height of the waves can easily be measured. This has been done conscientiously by several observers. It is by far harder to see the changes in the form of the catacrotic branch, and only a few keen observers have seen them. These changes of the pulse curve under the influence of feelings were proved as facts by experiments, but their interpretation was doubtful. With the exception of the rapidity of the heart-beats, which could easily be observed in some other way, all the symptoms of the influence of feelings on circulation are ambiguous. A difference in height of the single waves may be due to a change in the amount of blood which is pumped into the artery, but it also may be due to a change in the amplitude of the vibrations of the artery. The form of the catacrotic part of the sphygmographic curve may be changed by a different state of innervation of the arterial wall, but it also may be due to an increase or decrease of the friction of the surrounding tissues. The general rise or fall of the curve may indicate a change in the amount of blood which leaves the left ventricle, but it also may indicate a change in the amount of the capillary outflow.

The problem, nevertheless, is fully determined, and a solution is suggested by the constructions in Fig. 5. The form of the resulting movement depends, first, on the length of the line AB, secondly, on the length of the line AE, and thirdly, on the nature of the elastic movement. An elastic movement is determined if three constants are known, one of which is the amplitude, the second the friction, and the third the elasticity. Only AB can be measured directly, and there remain four unknown quantities to be determined. Four measurements must be sufficient for this purpose. It is obvious, however, that not any four measurements will do, but a method can be devised

[1] The amount of change in the base-line is the chief difference between the sphygmograms and the plethysmograms. It was stated recently that for this reason the plethysmograph could not be used for psychological experiments. An analysis of the mechanical conditions of these two instruments shows that also the sphygmogram must show some plethysmographic influences, and the author supplied experimental evidence for this result.

by which it is possible to determine each one of these four quantities. The problem can be solved in every case provided that the sphygmogram is trustworthy enough to justify the work. The length AB is proportional to the time of one heart-beat, and the length of the line AE is proportional to the amount of blood pumped into the arteries. The successful analysis of the pulse curves, therefore, shows changes of the action of the heart and makes it possible to distinguish them from the changes at the periphery.

Besides the length of the heart-beats there are invariably these four quantities which must be determined by the analysis of the pulse curves: Amount of incoming blood, amount of outflowing blood, elasticity of the artery, and friction of the tissues. These quantities depend on the action of the heart, the peripheral resistance, and the state of innervation of the artery. It is not possible to discuss here the bearing of this theory and of the facts which may be connected with it, on the different views of the localization and operation of the centres which control these functions. Anatomical and physiological evidence, however, leaves no doubt that the function of the heart and the innervation of the arteries and capillaries *are* under the control of nervous centres. It may be supposed, therefore, that changes of the pulse curve like those due to the influence of feelings are the effect of the function of these centres. It is to be expected that the detailed analysis of the pulse curves may give some indications as to the nature of this influence, for it may be observed how the function of these centres changes under the influence of mental processes.

A complete analysis of the physiological accompaniments of a feeling process must give a description of the changes in the function of the heart and the system, besides a description or at least enumeration of the other changes which can be observed. By a number of such investigations material for a general theory of physiological accompaniments of feelings may be obtained, which would not be void of interest for the psychology of feelings. Such a theory must contain the answers to the following questions: (1) How do the physiological reactions depend on the sense-stimulus? (2) How many possible circulatory reactions are there? (3) What is the location and interdependence of the respective physiological centres? The first question cannot so far be answered in general, but it will be possible to give a general answer when a greater number of systematic investigations on the effect of sense-stimuli have been carried on. Papers like those of Mentz may settle the question for certain sense-stimuli. From the results which have been obtained so far it comes out clearly that the

reaction does not depend merely on the nature of the stimulus, but that it depends largely on the psychical and physiological state of the subject. The answer to the second question may be given readily, but it seems advisable to give it in connection with an experimental investigation. It may be said, nevertheless, that the number of typical reactions is rather limited. The third problem, by its nature, cannot be definitely answered before the location of the respective centres is ascertained and their interdependence explained.

It is, finally, a merit of this theory of the pulse curves that it shows how the form of this curve may depend on central processes. The problem of the mysterious influence of mental processes is thus reduced to the analysis of merely physiological conditions. The theories on the nature of this influence are so numerous that they may well be called innumerable, and they vary from accepting a direct influence of ideas on the circulation to considering the body as a sounding-board which by every sensation is shaken in all its parts. Each one of these theories is also a theory of feelings, and a more or less exact description of these changes has been often taken for a descriptive psychology of feelings. The example of the sounding-board is taken from one of those papers which expound the theory that bodily changes follow directly on the perception, and that our sensation of these facts is the emotion. Every one of these bodily changes, whatsoever, is perceived, acutely or obscurely, the moment it occurs. This theory is defended by the argument that if we try to abstract from consciousness all the sensations of our bodily symptoms, we find we have nothing left behind. This argument, which may be found in almost every paper that deals with this theory, is remarkable, because it sometimes is referred to processes of every description, and thus comes into contradiction with psychophysical parallelism which excludes the acceptance of psychical states which have no physical correlate. This theory, as will have been noticed, is the theory of feelings expounded by James, Lange, Ribot, and others. It is widely accepted, and may be found also in books of popular or semi-popular nature. Two observations must be made against this view:

First, a perception of a bodily change which is felt in the moment the change occurs exists only in the theory, every real process needing a certain time. This point of the theory may be improved by admitting that the afferent process lasts as long as any other of the physiological processes of this kind. Either assumption, however, is contradicted by the experimental evidence supplied by Lehmann that the physiological changes occur after the beginning of an emotional state.

Secondly, if the theory refers only to those bodily changes which we know, it certainly is not true, for emotional states are sometimes observed without it being possible to find with modern instruments any bodily accompaniments. If the theory refers to bodily changes of every description, it is certainly true, or, better, it is beyond all attack because it becomes identical with psychophysical parallelism. In this general form this theory of feelings is as good as no theory at all, because it refers to mental states of every description.[1]

This conception of emotional states of mind as perceptions of bodily sensations would hardly have been promulgated, if the authors had tried to base it on experiments performed in the laboratory. An emotion but not the feeling-tone of a simple sensation may be mistaken for the sum of bodily sensations. It is, furthermore, remarkable that the promoters of this theory do not make a clear distinction between sensation and feeling. They introduce an emotional element by calling the perception of bodily changes a feeling of these changes. Only in this way do they succeed in building up emotional states of mind out of elements which are seemingly sensational. This does not succeed if the word feeling is replaced by the word sensation. The failure of this theory is due to two facts, first to the starting from a philosophical doctrine, and second to the lack of a precise distinction between feeling and sensation. It cannot be doubted after the above discussion how a definition of this difference may be given which holds for every empirical investigation.

A sense-stimulus produces a complex of nervous and central processes. Among these is a certain group of processes which manifest themselves by changing the innervation of the heart, the blood-vessels, the lungs, and certain muscles. Another group is formed by those nervous and central processes which are more or less immediate effects of the sense-stimulation. The first group of processes is referred subjectively to an emotional state of mind, and the second to a cognitive process; the first group of processes is the physiological accompaniment of feelings, the second that of sensations. The relative independence of the first group from the second group is warranted by the fact that

[1] More recent publications have taken this view. Cohn speaks of "Organgefühle des Gehirns," approaching Meynert's view on the causes of pleasure and pain. (P. Cohn: Gemüthserregungen und Krankheiten, pp. 23 and 50, 1903.) Cohn's book shows clearly that this theory belongs to the type of philosophical explanations. This is also suggested by Duprat who remarked the parallelism between the theories of James, Lange and Ribot, and the theories of certain Greek philosophers. (Duprat: "La psycho-physiologie des sentiments dans la philosophie ancienne, Archiv f. Geschichte d. Philosophie, vol. 18 (3), p. 395, 1905.)

the same processes are observed as accompaniments of ideational processes. A strict limit between these two groups of processes can be drawn when the central processes are better known, because to the first group belong all those processes which are found to be accompaniments as well of sensational as of ideational processes. In different sensations the emotional process may be more or less marked, and in others the cognitive process may be prominent, but it seems that feelings are an invariable accompaniment of the sensation. This suggests the definition of feelings as psychic processes, the physiological accompaniment of which are central processes which depend largely on the state of the organism, and which manifest themselves by changes in the innervation of the heart, the blood-vessels, the lungs, and muscles. The impossibility of directly comparing the sensations of different subjects is recognized, and it is also impossible to compare feelings, because in either case we are dealing with psychic processes.

THE MUTUAL INFLUENCE OF FEELINGS

BY JOHN A. H. KEITH

THE object of this investigation was to ascertain the mutual influence of simultaneous stimuli that appealed to different senses with regard to the *intensity* of their feeling values. The investigation covers combinations: (1) of colors and active touches, (2) of colors and passive touches, (3) of tones and active touches, (4) of tones and passive touches, (5) of colors and tones.

The basis of appreciation was a numerical scale[1] as follows:

1. Very disagreeable.
2. Disagreeable.
3. Slightly disagreeable.
4. Indifferent.
5. Slightly agreeable.
6. Agreeable.
7. Very agreeable.

The color series began with the one hundred thirty-six colors as put out by the Milton Bradley Co. This series consists of ninety pure spectrum colors, ten whites, blacks, and grays, and thirty-six broken spectrum colors. The colors were exposed at the back of a semicircular black-lined box for about two seconds. The subject was seated at a convenient distance, about three and a half feet, from the colors. In order to have a constant light, all experiments were conducted in a dark room with an electric light suspended over the subject's head. The whole series was used for ten times in order to get the range of judgments. Then twenty-eight colors, covering as fully as possible the range from 1 to 7, were selected for further experiment in combination.

At the same time a series of thirty-six touches, from velvet to

[1] Attention may be called to the fact that this paper arranges the conventional seven degrees of feelings in an order opposite to that of the other papers of this volume; it follows still the earlier traditions of our laboratory, while the more recent investigations call very disagreeable 7 and very agreeable 1; the indifference point remains the same. — EDITOR.

sandpaper, was being employed as the colors were. From this number fourteen were finally selected.

Similarly, by using a reed box, with reeds ranging from 128 to 1024 vibrations per second and separated from each other by four vibrations, from a much larger series twenty-seven tone-combinations were finally selected.

Moreover, from time to time, each selected series was given alone; and on the basis of these readings, averaging from thirty to forty, the "standard" for each stimulus was made. Tables I to III give a brief description of the stimuli and also the "standards" for each of two subjects, F. and M.

TABLE I. COLORS

No. of Color.	*Description.*	*Standard for F.*	*Standard for M.*
1	Violet Red. Tint no. 1	5.90	4.00
2	Red. Tint no. 1	6.00	6.00
3	Red	6.00	5.40
4	Orange Red	5.60	6.20
5	Red Orange	4.20	5.20
6	Yellow Orange. Tint no. 1	3.10	4.50
7	Yellow. Tint no. 1	2.30	5.20
8	Yellow. Shade no. 2	2.00	4.00
9	Green Yellow	2.80	5.60
10	Yellow Green	4.45	6.20
11	Green. Shade no. 1	5.70	4.40
12	Blue Green. Tint no. 1	3.40	6.00
13	Green Blue	5.00	3.50
14	Blue. Shade no. 1	5.40	2.10
15	Blue Violet	5.10	5.00
16	Violet. Tint no. 2	4.20	5.30
17	Violet. Shade no. 2	5.50	3.50
18	Red Violet	5.65	3.70
19	Black	4.00	4.00
20	Green Gray. no. 1	2.20	4.20
21	Green Gray. no. 2	2.00	4.00
22	A-Red. Light	3.70	3.70
23	A-Red. Dark	2.10	2.70
24	A-Orange. Dark	2.00	3.00
25	A-Yellow Orange. Light	3.10	4.30
26	A-Green Yellow. Dark	2.50	4.00
27	A-Blue Green. Medium	2.40	4.30
28	A-Violet. Medium	4.10	4.50
	Totals	122.40	124.50
	Average	4.35	4.44

In connection with this table it may be noted that the subjects agree regarding 2, 19, and 22, red tint no. 1, black, and A-red light; that M. estimates the colors higher than F. in seventeen cases; and that F. estimates 1, 3, 11, 13, 14, 15, 17, 18, higher than M. does. These individual differences are probably explicable on grounds of association; they are not, however, connected with the problem under consideration here, for we are concerned with the effect of combinations with other stimuli.

Table II. Touches

The various articles were fastened to small pieces of wood and placed in small cardboard boxes. In active touch, the subjects were allowed to stroke the object gently twice, always with a contracting movement of the forefinger of the right hand. In passive touch, the operator stroked the subject's forefinger with the object, twice as before. The table explains itself.

		Standards.			
No. of		*Active.*		*Passive.*	
Touch.	*Description.*	*F.*	*M.*	*F.*	*M.*
1	Thick napped velvet	6.90	6.60	7.00	7.00
2	Thin stretched rubber, such as is used on tambours	5.70	5.40	6.00	6.00
3	Glazed thin cardboard	5.80	5.50	5.50	6.00
4	White silk ribbon — always stroked with the ribs	5.80	5.70	5.40	5.00
5	Soft, split, rough leather	5.50	5.50	5.00	6.00
6	Smooth polished cork	5.70	5.10	5.40	6.00
7	Glazed tin	5.00	4.50	6.00	6.00
8	Rough, tarred paper	4.30	4.60	4.00	5.00
9	Blue blotting paper	4.50	5.10	4.20	5.00
10	Sand paper no. 1, fine grained	2.20	3.50	2.00	4.00
11	Shot no. 3, set in paraffine	2.70	4.90	3.00	3.50
12	Sandpaper no. 2½, coarse-grained	1.40	2.00	1.50	3.00
13	A coarse, rough, ridged cotton cloth, always stroked across the ridges	4.00	2.40	4.70	3.50
14	A thin, closely woven white muslin	4.20	4.60	4.20	5.00
	Totals	63.70	59.40	63.90	72.00
	Average	4.55	4.24	4.56	5.14

Table III. Tones

The table of tone-combinations shows that some are simply repetitions of the same chord in a higher octave, as 1 and 9, 5 and

13, 15 and 14. The first sixteen are harmonious; so also the twenty-sixth; the others introduce beats and discords, some of which are agreeable, as 20 and 21, while others are disagreeable, as 19 and 27. Individual differences appear in this as in the previous series. The totals introduced into the tables simply go to show that in each series the total judgments are not widely diverse.

No. of Tones.	*Description.*	*Standards.* *F.*	M.
1	256, 320	4.25	3.25
2	256, 384	4.25	3.00
3	256, 320, 384	5.60	3.00
4	320, 384	4.30	3.50
5	256, 512	4.65	5.50
6	320, 512	4.10	5.00
7	384, 512	4.90	5.00
8	512, 640	5.40	5.30
9	512, 768	5.50	5.80
10	640, 768	4.40	5.00
11	640, 1024	4.25	3.40
12	768, 1024	4.80	5.50
13	512, 1024	5.60	5.80
14	512, 640, 768, 1024	6.30	5.90
15	256, 320, 384, 512	6.50	4.00
16	320, 512, 768	5.00	4.70
17	136, 144	2.00	2.50
18	156, 160	3.80	4.00
19	136, 140	2.70	3.50
20	440, 444	6.81	6.00
21	504, 508	6.70	6.40
22	148, 152, 156	1.70	2.50
23	172, 296, 452	3.00	2.50
24	180, 480, 768	3.40	2.50
25	232, 328, 492	3.20	2.50
26	256, 512, 768	5.00	5.50
27	256, 240, 384	2.00	1.70
	Totals	117.04	113.25
	Average	4.33	4.20

The following tables deal with the combined series.

Table IV shows that the appreciation of the colors was, in general, lowered slightly by the combinations with the tones; and, also, that the appreciation of the tones was lowered more than one point by the combinations with the colors. By referring to Tables I and III, M.'s average for the colors is 4.44 and for the tones 4.20.

TABLE IV. COLOR-TONE RESULTS. M.

No. of Color	No. of times color was Raised	Lowered	Not affected	Total no. of points color was Raised +	Lowered −	Net Result +	Net Result −
1	3	15	9	3	15		12
2	0	22	5	0	23		23
3	0	27	0	0	32.2		32.2
4	0	27	0	0	20.4		20.4
5	0	27	0	0	8.2		8.2
6	23	4	0	12.5	2	10.5	
7	14	13	0	11.2	2.6	8.6	
8	6	5	16	6	5	1	
9	16	11	0	6.4	6.6		.2
10	1	26	0	.8	20.2		19.4
11	0	27	0	.0	36.8		36.8
12	0	23	4	0	23		23
13	2	25	0	1	18.5		17.5
14	9	18	0	8.1	9		.9
15	1	12	15	1	14		13
16	1	26	0	.7	16.8		16.1
17	7	20	0	6.5	12.		5.5
18	13	14	0	4.9	10.8		5.9
19	0	0	27	0	0		
20	1	26	0	.8	9.2		8.4
21	0	6	21	0	6		6
22	7	20	0	3.1	16		12.9
23	21	6	0	9.3	4.2	5.1	
24	12	2	13	17	3	14	
25	8	19	0	5	7.7		2.1
26	1	4	22	1	4		3
27	16	11	0	12	3.3	8.9	
28	13	14	0	6	10		3.5
Totals	175	450	131			48.1	270.

Grand total (28×27) 756. Net lowered 221.9

Average lowering of each color judgment, $\frac{221.9}{756} = .293$

% of judgments of color lowered, $\frac{450}{756} = 59+$

% of judgments of color raised, $\frac{175}{756} = 23+$

% of judgments of color not affected, $\frac{131}{756} = 17+$

No. of Tone	No. of times tone was Raised	Lowered	Not affected	Total No. of points tone was Raised +	Lowered −	Net Result +	Net Result −
1	0	28	0	0	29		29
2	4	9	15	4	9		5
3	2	22	4	2	28		26
4	7	21	0	4.5	17.5		13
5	0	28	0	0	32		32
6	0	24	4	0	35		35
7	0	22	6	0	32		32
8	4	24	0	2.8	11.2		8.4
9	3	25	0	.6	37		36.3
10	0	17	11	0	25		25
11	15	13	0	16	7.2	8.8	
12	6	22	0	3	20		17
13	10	18	0	2	22.4		20.4
14	3	25	0	.3	37.5		3.72
15	0	27	1	0	55		55
16	4	24	0	1.2	38.8		37.6
17	2	26	0	1	22		21
18	0	27	1	0	66		66
19	0	28	0	0	56		56
20	1	19	8	1	23		22
21	6	22	0	3.6	13.8		10.2
22	0	28	0	0	37.		37
23	1	27	0	.5	33.5		33
24	10	18	0	5	13		8
25	7	21	0	3.5	24.5		21
26	5	23	0	2.5	50.5		48
27	1	27	0	.3	18.9		18.6
Totals	91	615	50			8.8	829.7

Grand total (27×28), 756. Net lowered 820.9

Average lowering of each tone judgment, 1.08+

% of judgments of tones lowered, $\frac{615}{756} = 81+$

% of judgments of tones raised, $\frac{91}{756} = 12+$

% of judgments of tones not affected, $\frac{50}{756} = 6+$

TABLE V. COLOR-TONE RESULTS. F.

No. of Color	No. of times color was Raised	Lowered	Not affected	No. of points color was Raised +	Lowered —	Net result +	—
1	9	18	0	10	19.2		9.2
2	0	24	3	0	38		38
3	0	26	1	0	31		31
4	1	26	0	0.4	33.6		33.2
5	9	18	0	7.2	16.6		9.4
6	3	24	0	3.7	12.4		8.7
7	13	14	0	9.1	4.2	4.9	
8	9	4	13	9	4	5	
9	21	6	0	12.4	5.8	6.6	
10	18	9	0	15.90	12	3.9	
11	19	8	0	10.7	5.6	5.1	
12	9	18	0	7.4	13.2		5.8
13	5	7	15	6	7		1
14	7	20	0	6.2	4	2.2	
15	5	22	0	6.5	5.1	1.4	
16	1	26	0	.8	17.2		16.4
17	2	25	0	1	13.5		12.5
18	3	24	0	1.35	18.6		17.25
19	0	3	24	0	3		3
20	18	9	0	14.4	2.8		11.6
21	9	0	18	9	0	9	
22	5	22	0	4.5	26.2		21.7
23	2	25	0	1.8	5.5		3.7
24	5	7	15	6	7		1
25	1	26	0	.9	28.4		27.5
26	16	11	0	8	5.5	2.5	
27	19	8	0	14.4	3.2	11.2	
28	7	20	0	6.3	4	2.3	
Totals	215	451	90			54.1	250.95
	Grand total	756			Net lowered		196.85

Average lowering of each color judgment, .26+

% of judgments of color lowered, $\frac{451}{756}=59.6+$

% of judgments of color raised, $\frac{215}{756}=28+$

% of judgments of color not affected, $\frac{90}{756}=11.9+$

No. of Tone	No. of times tone was Raised	Lowered	Not affected	No. of points tone was Raised +	Lowered —	Net result +	—
1	6	22	0	3	20.5		17.5
2	2	26	0	1.5	22.5		21
3	2	26	0	.8	24.6		23.8
4	2	26	0	1.4	16.8		15.4
5	4	24	0	1.4	27.6		26.2
6	3	25	0	6.7	26.5		19.8
7	4	24	0	1.4	40.6		39.2
8	1	27	0	.6	32.8		32.2
9	1	27	0	.5	34.5		34
10	8	20	0	5.8	25		19.2
11	3	25	0	2.25	33.25		31
12	4	24	0	.8	38.2		37.4
13	2	26	0	.8	23.6		22.8
14	2	26	0	1.4	25.8		24.4
15	1	27	0	.5	32.5		32
16	0	21	7	0	40		40
17	4	10	14	4	10		6
18	18	10	0	22.6	14	8.2	
19	17	11	0	22.1	13.7	8.4	
20	6	22	0	4.8	22.6		17.8
21	10	18	0	3	17.6		14.6
22	18	10	0	10.4	7	3.4	
23	1	20	7	2	21		19
24	0	28	0	0	20.4		20.4
25	3	25	0	2.4	21		18.6
26	0	22	6	0	39		39
27	1	11	16	1	11		10
Totals	123	583	50			20	581.3
	Grand total		756		Net lowered,		581.3

Average lowering of each tone judgment, .742+

% of judgments of tones lowered, $\frac{583}{756}=77+$

% of judgments of tones raised, $\frac{123}{756}=16+$

% of judgments of tones not affected, $\frac{50}{756}=6+$

TABLE VI. TONE-ACTIVE TOUCH RESULTS. M.

No. of Tone	*No. of times tone was* Raised	Lowered	Not affected	*No. of points tone was* Raised+	Lowered−	*Net Result* +	−
1	14	0	0	31.5	0	31.5	
2	14	0	0	29	0	29	
3	14	0	0	30	0	30	
4	14	0	0	32	0	32	
5	9	5	0	9.5	4.5	5	
6	14	0	0	25	0	25	
7	14	0	0	22	0	22	
8	14	0	0	22.8	0	22.8	
9	14	0	0	16.8	0	16.8	
10	14	0	0	25	0	25	
11	14	0	0	46.8	0	46.8	
12	14	0	0	21	0	21	
13	14	0	0	13.8	0	13.8	
14	13	1	0	13.4	.9	12.5	
15	13	0	1	23	0	23	
16	14	0	0	24.2	0	24.2	
17	14	0	0	25	0	25	
18	13	0	1	28	0	28	
19	14	0	0	30	0	30	
20	13	1	0	13	0	13	
21	13	1	0	7.8	0	7.8	
22	14	0	0	24	0	24	
23	14	0	0	21	0	21	
24	14	0	0	42	0	42	
25	14	0	0	38	0	38	
26	12	2	0	11	2	9	
27	13	1	0	22.9	.6	22.3	
Totals,	365	11	2			649.5	

Grand total, 378. Net raised, 649.5

Average raising of each tone judgment, 1.71+

% of judgments of tones lowered, $\frac{11}{378}=.2+$

% of judgments of tones raised, $\frac{365}{378}=97+$

% of judgments of tones not affected, $\frac{2}{378}=.5+$

No. of Touch	*No. of times touch was* Raised	Lowered	Not affected	*No. of points touch was* Raised+	Lowered−	*Net Result* +	−
1	27	0	0	10.8	0	10.8	
2	6	21	0	3.6	12.4		8.8
3	10	17	0	5.8	8.5		3.5
4	6	21	0	2.8	16.7		13.9
5	14	13	0	8	6.5	1.5	
6	8	19	0	7.2	6.9	.3	
7	16	11	0	11	6.5	4.5	
8	22	5	0	13.8	3	10.8	
9	7	20	0	6.3	5	1.3	
10	26	1	0	23	.5	22.5	3.3
11	21	6	0	2.1	5.4		
12	26	0	1	38	0	38	
13	27	0	0	48.4	0	48.4	
14	23	4	0	14.2	2.4	11.8	
Totals	239	138	1			149.9	29.5

Grand total, 378 Net raised, 120.4

Average raising of each active touch judgment, .31+

% of judgments of active touch lowered, $\frac{138}{378}=36+$

% of judgments of active touch raised, $\frac{239}{378}=63+$

% of judgments of active touch not affected, $\frac{1}{378}=.2+$

Under combination influences, the average is reduced to 4.14+ for colors, and 3.12 for tones.

The next Table, V, shows the color-tone results for F. as Table IV showed them for M. F.'s average for colors (Table I) alone was 4.35, and was reduced in combination with tones by .26, or to 4.19. So, also, F.'s average for tones alone (Table III), 4.33, was reduced by .74+ to 3.59. The averages in both cases show the same general tendency to a lowering of the appreciation in both series when the series are combined, but the tones are lowered more than the colors.

Table VI shows the effect of combining tones and active touches as reported by M.

The effect of this combination is clear and unmistakeable. The appreciation of the tones is raised 1.71+ points; and of the active touches, .31+ points. This result is the opposite of that shown in Table IV, where colors and tones were combined. There is this agreement, however, that the appreciation of the tones is changed more than that of the other stimuli. Relatively, the appreciation of the touches changes least.

Table VII shows the effects on F. of combining tones and active touches. The same general tendencies appear as in the case of M.; but the changes in appreciation are not so marked. This is not easy to explain, for F. estimated both the active touches and the tones higher when taken alone than did M.

Table VIII shows the effect on F. of combining tones and passive touches. The same general tendency to increased appreciation appears, but the tones are raised more and the touches raised less than in Table VII. This may be explained, perhaps, on the basis of increasing appreciation with increased participation.

Tables IX and X show the effect on M. and F., respectively, of combining colors and active touches. M. estimates both slightly higher, while F. estimates both slightly lower. This difference cannot be explained by the standards for each subject. From Tables I and III we get:

Standards.

	Colors.	*Active Touches.*
M.	4.44	4.24
F.	4.35	4.55

Vith M. the colors go up to	4.87
Vith F. the colors go down to	4.22
With M. the active touches go up to	4.41
With F. the active touches go down to	4.17

TABLE VII. TONE-ACTIVE TOUCH RESULTS. F.

No. of Tone	No. of times tone was Raised	Lowered	Not affected	No. of points tone was Raised +	Lowered −	Net Results +	−
1	11	3	0	9.25	1.75	7.5	
2	10	4	0	9.5	1	8.5	
3	7	7	0	5.8	7.2		1.4
4	10	4	0	12	3.2	8.8	
5	9	5	0	9.85	4.25	5.6	
6	9	5	0	15.1	4.5	10.6	
7	10	4	0	8	6.6	1.4	
8	7	7	0	5.2	6.8		1.6
9	10	4	0	8	9		1
10	12	2	0	14.2	2.8	11.4	
11	12	2	0	16	2.5	13.5	
12	13	1	0	9.6	.8	8.8	
13	13	1	0	12.2	.6	11.2	
14	10	4	0	7	2.2	4.8	
15	6	8	0	3	4	1	
16	9	1	4	11	2	9	
17	9	0	5	14	0	14	
18	12	2	0	16.4	2.6	13.8	
19	12	2	0	27.6	2.4	25.2	
20	12	2	0	2.4	2.6		.2
21	14	0	0	4.2	0	4.2	
22	14	0	0	17.2	0	17.2	
23	4	5	5	6	5	1	
24	0	14	0	0	9.6		9.6
25	7	5	2	5.6	6		.4
26	6	1	7	8	1	7	
27	11	0	3	14	0	14	
Totals,	259	93	26			186.5	15.2

Grand total, 378 Net raised, 171.3

Averaging raising of each tone judgment, .45

% of judgments of tones lowered, $\frac{93}{378}=24+$

% of judgments of tones raised, $\frac{259}{378}=68+$

% of judgments of tones not affected, $\frac{26}{378}=7-$

No. of Touch	No. of times touch was Raised	Lowered	Not affected	No. of points tone was Raised +	Lowered −	Net Result +	−
1	22	5	0	2.2	6.5		4.3
2	16	11	0	8.8	8.7	.1	
3	20	7	0	7	5.6	1.4	
4	19	8	0	5.8	11.4		5.6
5	18	9	0	14	7.5	6.5	
6	16	11	0	6.8	9.7		2.9
7	13	5	9	13	5.	8	
8	13	14	0	13.1	12.2	.9	
9	19	8	0	14.5	5	9.5	
10	9	18	0	8.2	7.6	.6	
11	22	5	0	8.6	3.5	5.1	
12	15	12	0	15	4.8	10.2	
13	22	1	4	35	1	34	
14	20	7	0	26	1.4	24.6	
Totals,	244	121	13			100.9	12.8

Grand total, 378 Net raised, 88.1

Average raising of each active touch judgment, .23+

% of judgments of active touch lowered, $\frac{121}{378}=32+$

% of judgments of active touch raised, $\frac{244}{378}=64+$

% of judgments of active touch not affected, $\frac{13}{378}=3+$

TABLE VIII. TONE-PASSIVE TOUCH RESULTS. F.

No. of Tone	No. of times tone was Raised	Lowered	Not affected	No. of points tone was Raised +	Lowered −	Net Result +	−
1	13	1	0	17.75	.25	17.5	
2	12	2	0	18	0	18	
3	13	1	0	11.2	.6	10.6	
4	14	0	0	14.8	0	14.8	
5	11	3	0	6.65	1.95	4.7	
6	9	5	0	12.1	1.50	10.6	
7	13	1	0	6.3	.9	5.4	
8	14	0	0	17.4	0	17.4	
9	6	8	0	5	4	1	
10	14	0	0	20.4	0	20.4	
11	14	0	0	12.5	0	12.5	
12	10	4	0	7	5.2	1.8	
31	14	0	0	10.6	0	10.6	
14	12	2	0	8.4	.6	7.8	
15	8	6	0	4	3	1	
16	1	2	11	1	2	1	
17	4	0	10	4	0	4	
18	5	9	0	7	8.2		1.2
19	9	5	0	2.7	4.5		1.8
20	13	1	0	2.6	.8	1.8	
21	13	1	0	3.9	.7	2.2	
22	13	1	0	8.9	.7	8.2	
23	6	2	6	10	2	8	
24	12	2	0	15.2	.8	14.4	
25	11	3	0	15.8	.6	15.2	
26	0	6	8	0	8		8
27	10	0	4	13	0	13	
Totals,	274	65	39			211.7	11.0

Grand total, 378 Net raised, 200.7

Average raising of each tone judgment, .52

% of judgments of tones lowered, $\frac{65}{378}=17+$

% of judgments of tones raised, $\frac{274}{378}=72+$

% of judgments of tones not affected, $\frac{39}{378}=10+$

No. of Touch	No. of times touch was Raised	Lowered	Not affected	No. of points touch was Raised +	Lowered −	Net Result +	−
1	0	10	17	0	10		10
2	4	13	10	4	16		12
3	11	16	0	6.5	12		5.5
4	13	14	0	12.8	9.6	3.2	
5	12	5	10	17	8	9	
6	12	15	0	7.2	12		4.8
7	3	12	12	3	14		11
8	14	2	11	17	2	15	
9	11	16	0	12.8	20.2		7.4
10	19	1	7	19	1	18	
11	5	2	20	5	2	3	
12	27	0	0	34.5	0	34.5	
13	14	13	0	8.2	9.1		.9
14	17	10	0	25.6	2	23.6	
Totals,	162	129	87			106.3	51.6

Grand total, 378 Net raised, 54.7

Average raising of each passive touch judgment, .14

% of judgments of passive touch lowered, $\frac{129}{378}=34$

% of judgments of passive touch raised, $\frac{162}{378}=42$

% of judgments of passive touch not affected, $\frac{87}{378}=23$

TABLE IX. COLOR-ACTIVE TOUCH RESULTS. M.

No. of Color	No. of times color was Raised	Lowered	Not affected	No. of points color was Raised +	Lowered −	Net result +	−
1	5	1	8	5	1	4	
2	0	7	7	0	7		7
3	3	11	0	1.8	4.4		2.6
4	3	11	0	2.4	4.2		1.8
5	3	11	0	2.4	2.2	.2	
6	12	2	0	6	1	5	
7	2	12	0	1.6	2.4		.8
8	12	0	2	12	0	12	
9	13	1	0	14.2	.6	13.6	
10	7	7	0	5.6	2.4	3.2	
11	11	3	0	6.6	1.2	5.4	
12	7	1	6	7	1	6	
13	9	5	0	5.5	2.5	3	
14	11	3	0	13.9	.3	13.6	
15	11	0	3	12	0	12	
16	10	4	0	8	1.2	6.8	
17	12	2	0	16	1	15	
18	14	0	0	24.8	0	24.8	
19	0	0	14	0	0		
20	9	5	0	7.2	1	6.2	
21	1	0	13	1	0	1	
22	14	0	0	5.2	0	5.2	
23	14	0	0	10.2	0	10.2	
24	8	0	6	14	0	14	
25	9	5	0	6.3	1.5	4.8	
26	4	1	9	4	1	3	
27	11	3	0	4.3	.9	3.4	
28	13	1	0	10.5	.5	10	
Totals,	228	96	68			182.4	12.2

Grand total, 392 Net raised, 170.2

Average raising of each color judgment, .43

% of judgments of colors lowered, $\frac{96}{392}=24+$

% of judgments of colors raised, $\frac{228}{392}=58+$

% of judgments of colors not affected, $\frac{68}{392}=17+$

No. of Touch	No. of times touch was Raised	Lowered	Not affected	No. of points touch was Raised +	Lowered −	Net Result +	−
1	8	20	0	3.2	15		11.8
2	4	24	0	3.4	11.6		8.2
3	18	10	0	10	5	5	
4	8	20	0	3.4	23		19.6
5	8	20	0	5	13		8
6	2	26	0	1.8	8.6		6.8
7	25	3	0	26.5	4.5	22	
8	21	7	0	10.4	4.2	6.2	
9	3	25	0	2.7	8.5		5.8
10	27	1	0	33.5	.5	33	
11	23	5	0	3.3	4.5		1.2
12	22	0	6	41	0	41	
13	26	2	0	42.6	.8	41.6	
14	19	9	0	9.6	8.4	1.2	
Totals	214	172	6			130.0	61.4

Grand total, 392 Net raised, 68.6

Average raising of each active touch judgment, .17+

% of judgment of active touch, lowered, $\frac{172}{392}=43+$

% of judgments of active touch raised, $\frac{214}{392}=54+$

% of judgments of active touch not affected, $\frac{6}{392}=1.8+$

TABLE X. COLOR-ACTIVE TOUCH RESULTS. F.

No. of Color	No. of times color was Raised	Lowered	Not affected	No. of points color was Raised +	Lowered −	Net Result +	−
1	13	1	0	8.3	.9	2.4	
2	5	4	5	5	6		1
3	2	5	7	2	5		3
4	9	5	0	7.6	3	4.3	
5	9	5	0	16.2	7	9.2	
6	0	14	0	0	20.4		20.4
7	3	11	0	2.1	5.3		3.2
8	1	3	10	1	3		2.0
9	5	9	0	3	8.2		5.2
10	9	5	0	10.95	8.25	2.7	
11	12	2	0	8.6	1.4	7.2	
12	8	6	0	9.8	2.4	7.4	
13	4	1	9	6	1	5	
14	10	4	0	11	1.6	9.4	
15	2	12	0	2.8	3.2		.4
16	2	12	0	1.6	7.4		5.8
17	8	6	0	5	4	1	
18	11	3	0	9.85	3.95	5.9	
19	0	0	14	0	0		
20	1	13	0	.8	14.4		13.6
21	2	7	5	2	7		5
22	8	6	0	11.4	9.2	2.2	
23	0	14	0	0	11.4		11.4
24	0	10	4	0	10		10
25	2	12	0	3.8	10.2		6.4
26	0	14	0	0	11		11
27	0	14	0	0	8.6		8.6
28	2	12	0	1.8	6.2		4.4
Totals,	129	209	54			56.7	111.4

Grand total, 392 Net lowered, 54.7

Average lowering of each color judgment, .13+

% of judgments of colors lowered, $\frac{209}{392} = 53+$

% of judgments of colors raised, $\frac{129}{392} = 32+$

% of judgments of colors not affected, $\frac{54}{392} = 13+$

No. of Touch	No. of times touch was Raised	Lowered	Not affected	No. of points touch was Raised +	Lowered −	Net Result +	−
1	11	17	0	1.1	28.3		27.2
2	14	14	0	11.2	13.8		2.6
3	19	9	0	9.8	7.2	2.6	
4	11	17	0	2.2	25.6		23.4
5	13	15	0	12.5	16.5		4
6	12	16	0	4.6	12.2		7.6
7	15	0	13	23	0	23	
8	2	26	0	.6	26.8		26.2
9	6	22	0	4	25		21
10	0	28	0	0	17.6		17.6
11	19	9	0	6.7	6.3	.4	
12	3	25	0	3.8	10		6.2
13	2	26	0	2	32		30
14	8	20	0	6.4	19		12.6
Totals,	135	244	13			26.0	178.4

Grand total, 392 Net lowered, 152.4

Average lowering of each active touch, .38+

% of judgments of active touch lowered, $\frac{244}{392} = 62+$

% of judgments of active touch raised, $\frac{135}{292} = 34+$

% of judgments of active touch not affected, $\frac{13}{392} = 3+$

TABLE XI. COLOR-PASSIVE TOUCH RESULTS. M.

No. of Color	No. of times color was Raised	Lowered	Not affected	No. of points color was Raised +	Lowered —	Ne +
1	9	1	4	10	1	9
2	7	2	5	7	2	5
3	7	7	0	4.2	2.8	1.4
4	6	8	0	4.8	1.6	3.2
5	9	5	0	11.2	1	10.2
6	14	0	0	21	0	21
7	13	1	0	14.4	.2	14.2
8	11	0	3	14	0	14
9	14	0	0	18.6	0	18.6
10	7	7	0	5.6	1.4	4.2
11	10	4	0	7	2.6	4.4
12	3	3	8	3	3	
13	13	1	0	12.5	.5	12
14	14	0	0	25.6		25.6
15	2	0	12	2	0	2
16	10	4	0	11	1.2	9.8
17	13	1	0	16.5	.5	16
18	13	1	0	13.9	.7	13.2
19	0	0	14	0	0	
20	13	0	1	12.4	0	12.4
21	7	0	7	7	0	7
22	13	1	0	11.9	.7	11.2
23	14	0	0	18.2	0	18.2
24	12	0	2	22	0	22
25	14	0	0	15.8	0	15.8
26	9	0	5	9	0	9
27	12	2	0	9.4	.6	8.8
28	10	4	0	6	2	4
Totals,	279	52	61			282.2

Grand total, 392 Net raised, 282.2

Average raising of each color judgment, .72—

% of judgments of colors lowered, $\frac{52}{392} = 13+$

% of judgments of colors raised, $\frac{279}{392} = 71+$

% of judgments of colors not affected, $\frac{61}{392} = 15+$

	−	+	−
	3		3
5	16		13
5	12		7
9	3	6	
6	3	3	
5	12		7
1	27		26
12	0	12	
23	0	23	
22	0	22	
24	0	24	
	0	19	
	3	29	
	0	22	
		160	56

Net raised, 104

Average raising of each passive touch judgment, .26+

% of judgments of passive touch lowered, $\frac{67}{392} = 17+$

% of judgments of passive touch raised, $\frac{171}{392} = 43+$

% of judgments of passive touch not affected, $\frac{154}{392} = 39+$

TABLE XII. COLOR-PASSIVE TOUCH RESULTS. F.

No. of Color	No. of times color was Raised	Lowered	Not affected	No. of points color was Raised +	Lowered −	Net Result +	−
1	14	0	0	12.4	0	12.4	
2	7	2	5	7	2	5	
3	1	1	12	1	1		
4	9	5	0	3.6	3	.6	
5	14	0	0	15.2	0	15.2	
6	0	14	0	0	2.4		2.4
7	13	1	0	9.1	.3	8.8	
8	7	0	7	9	0	9	
9	11	3	0	5.2	2.4	2.8	
10	11	3	0	12.05	4.35	7.7	
11	13	1	0	14.9	.7	14.2	
12	4	10	0	2.4	6		3.6
13	1	1	12	1	1		
14	11	3	0	6.6	1.2	5.4	
15	9	5	0	4.5	.5	4	
16	5	9	0	4	3.8	.2	
17	12	2	0	6	2	4	
18	13	1	0	12.55	.65	11.9	
19	0	0	14	0	0		
20	11	3	0	8.8	.6	8.2	
21	10	0	4	11	0	11	
22	10	4	0	14	1.2	12.8	
23	3	11	0	4.7	1.1	3.6	
24	2	0	12	2	0	2	
25	4	10	0	5.8	1	4.8	
26	7	7	0	4.5	3.5	1	
27	11	3	0	7.6	1.2	6.4	
28	12	2	0	10.8	.2	10.6	
Totals,	226	100	66			161.6	6

Grand total, 392 Net raised, 155.6

Average raising of each color judgment, .40 −

% of judgments of colors lowered, $\frac{100}{392} = 25+$

% of judgments of colors raised, $\frac{226}{392} = 57+$

% of judgments of colors not affected, $\frac{66}{392} = 16+$

No. of Touch	No. of times touch was Raised	Lowered	Not affected	No. of points touch was Raised +	Lowered −	Net Result +	−
1	0	8	20	0	8		8
2	3	14	11	3	15		12
3	23	5	0	14.5	2.5	12	
4	17	11	0	11.2	4.4	6.8	
5	21	1	6	27	1	26	
6	16	12	0	9.6	5.8	3.8	
7	4	10	14	4	10		6
8	13	7	8	14	7	7	
9	17	11	0	18.6	3.2	15.4	
10	15	1	12	15	1	14	
11	0	1	27	0	1		1
12	28	0	0	21	0	21	
13	21	7	0	18.3	7.9	10.4	
14	24	4	0	31.2	1.8	29.4	
Totals,	202	92	98			145.8	27

Grand total, 392 Net raised, 118.8

Average raising of each passive touch judgment, .30+

% of judgments of passive touch lowered, $\frac{92}{392} = 23+$

% of judgments of passive touch raised, $\frac{202}{392} = 51+$

% of judgments of passive touch not affected, $\frac{98}{392} = 25+$

Evidently no dynamic explanation of this difference is possible. It has been impossible to reveal the cause of this particular difference; in all other respects, however, the subjects agree in the general tendencies revealed.

There remain the combinations of colors and passive touches. These combinations are shown in Tables XI and XII.

In these tables the same general tendency to estimate both higher when colors and passive touches are combined appears. M. raises the colors more than F., and F. raises the touches more than M. This is perfectly regular, as the following table shows:

	Standards.		*Raised in Combination to*	
	Colors.	*Passive Touches.*	*Colors.*	*Passive Touches.*
M.	4.44	5.14	5.16	5.40
F.	4.53	4.56	4.75	4.85

The whole results are recapitulated for both M. and F. in Table XIII.

TABLE XIII. RECAPITULATION

		Average Standard	*% of judgments of (——) Raised*	*Lowered*	*Not affected*	*Av. + or − influence on each (——)*	
	F.						
1	Colors	4.35	28	59.6	11.9	(Color)	− .26+
	Tones	4.33	16	77	6	(Tone)	− .74+
2	Colors	4.35	32	53	13	(Colors)	− .13+
	A. Touches	4.55	34	62	3	(A. T.)	− .38+
3	Colors	4.35	57	25	16	(Colors)	+ .40+
	P. Touches	4.56	51	23	25	(P. T.)	+ .30+
4	Tones	4.33	68	24	7	(Tones)	+ .45+
	A. Touches	4.55	64	32	3	(A. T.)	+ .23+
5	Tones	4.33	72	17	10	(Tones)	+ .52+
	P. Touches	4.56	42	34	23	(P. T.)	+ .14+
	M.						
1	Colors	4.44	23	59	17	(Color)	− .29+
	Tones	4.20	12	81	6	(Tones)	−1.08
2	Colors	4.44	58	24	17	(Colors)	+ .43+
	A. Touches	4.24	54	43	1.8+	(A. T.)	+ .17+
3	Colors	4.44	71	13	15	(Colors)	+ .72+
	P. Touches	5.14	43	17	39	(P. T.)	+ .26
4	Tones	4.20	99	.2	.5	(Tones)	+1.71+
	A. Touches	4.24	63	36	.2	(A. T.)	+ .31+

TABLE XII. COLOR-PASSIVE TOUCH RESULTS. F.

No. of Color	No. of times color was Raised	Lowered	Not affected	No. of points color was Raised +	Lowered −	Net Result +	−
1	14	0	0	12.4	0	12.4	
2	7	2	5	7	2	5	
3	1	1	12	1	1		
4	9	5	0	3.6	3	.6	
5	14	0	0	15.2	0	15.2	
6	0	14	0	0	2.4		2.4
7	13	1	0	9.1	.3	8.8	
8	7	0	7	9	0	9	
9	11	3	0	5.2	2.4	2.8	
10	11	3	0	12.05	4.35	7.7	
11	13	1	0	14.9	.7	14.2	
12	4	10	0	2.4	6		3.6
13	1	1	12	1	1		
14	11	3	0	6.6	1.2	5.4	
15	9	5	0	4.5	.5	4	
16	5	9	0	4	3.8	.2	
17	12	2	0	6	2	4	
18	13	1	0	12.55	.65	11.9	
19	0	0	14	0	0		
20	11	3	0	8.8	.6	8.2	
21	10	0	4	11	0	11	
22	10	4	0	14	1.2	12.8	
	3	11	0	4.7	1.1	3.6	
	2	0	12	2	0	2	
	4	10	0	5.8	1	4.8	
	7	7	0	4.5	3.5	1	
	11	3	0	7.6	1.2	6.4	
		2	0	10.8	.2	10.6	
		100	66			161.6	6

total. 392 Net raised, 155.6

g of each color judgment, .40 −

colors lowered, $\frac{100}{392} = 25+$

colors raised, $\frac{226}{392} = 57+$

re not affected, $\frac{66}{392} = 16+$

No. of Touch	No. of times touch was Raised	Lowered	Not affected	No. of points touch was Raised +	Lowered −	Net Result +	−
1	0	8	20	0	8		8
2	3	14	11	3	15		12
3	23	5	0	14.5	2.5	12	
4	17	11	0	11.2	4.4	6.8	
5	21	1	6	27	1	26	
6	16	12	0	9.6	5.8	3.8	
7	4	10	14	4	10		6
8	13	7	8	14	7	7	
9	17	11	0	18.6	3.2	15.4	
10	15	1	12	15	1	14	
11	0	1	27	0	1		1
12	28	0	0	21	0	21	
13	21	7	0	18.3	7.9	10.4	
14	24	4	0	31.2	1.8	29.4	
Totals,	202	92	98			145.8	27

Grand total, 392 Net raised, 118.8

Average raising of each passive touch judgment, .30+

% of judgments of passive touch lowered, $\frac{92}{392} = 23+$

% of judgments of passive touch raised, $\frac{202}{392} = 51+$

% of judgments of passive touch not affected, $\frac{98}{392} = 25+$

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From this last table certain conclusions may be drawn.

(1) When Colors and Tones were combined, both were lowered in the appreciation of both subjects. The percentages show: (*a*) That about the same number of colors was lowered, 59.6 % for F. and 59 % for M. (*b*) That about the same average displacement of colors occurred, .26 + for F. and .29 + for M. (*c*) That about the same number of tones was lowered, 77 % for F. and 81 % for M. (*d*) That the tones were lowered more for M., − .74 + for F., and 1.08 + for M.

(2) When Colors and Active Touches were combined, for F. both are lowered; for M. both are raised. The colors are lowered only very slightly, .13 for F, while for M they are raised .43 + ; and conversely, the active touches are lowered .38 + for F, and raised only .17 + for M. Still, it appears clear that with F. there was an interference, — and both colors and active touches are lowered while the same combinations with M. are mutually reënforcing.

(3) When Colors and Passive Touches were combined, the appreciation of both was raised for both F. and M. The result shows: (*a*) That the percentages of colors and passive touches raised are practically the same for both F. and M. (*b*) That the color displacement is greater for M. than for F., being + .72 + for M. and only .40 + for F. (*c*) That there is only a slight difference in the displacement of the passive touches.

(4) When Tones and Active Touches were combined, the appreciation of both was raised for both F. and M. It appears (*a*) that the displacement for F. is very slight, .45 + and .23 +, when compared with the displacement for M., 1.71 + and .31 +. But, (*b*) that the displacement of tones is greater than that of the active touches for both F. and M. (*c*) That this same relatively great displacement of tones occurred in the opposite direction when colors and tones were combined.

(5) When Tones and Passive Touches were combined, the appreciation of both was raised for F, — the tones being raised more than the passive touches. This combination was not tried with M. for lack of time.

From time to time some special tests of these general tendencies were applied. From the results already set forth, one could predict that there was a strong probability that when the tone-series was combined with a constant touch-series, active or passive, the appreciation of the tones would be raised. This was tried by allowing the subject M. to rub his hand over the somewhat rough pillow of

a tilting board. The results showed that the appreciation of twenty-five out of twenty-seven tones was raised. Other predictions were similarly verified.

Of course, any experiment of this nature is exposed to a great many chances of error. The subjects may be fatigued, or depressed generally. But the wide range of different readings taken is a reasonable assurance that the chances of error are minimized. And it is to be noted, also, that individual differences of appreciation do not vitiate the results. In getting the "standards" no less than 2000 judgments were given by each subject, while for the tables each subject gave no less than 4000 judgments. The curves made from these data show the effect of each separate combination by their variation from the standard. The tables and analyses and conclusions already introduced show, in general, that *our appreciation of each of several stimuli in combination is different from our appreciation of the same stimuli when taken separately.* The results show that this appreciation may be either raised or lowered; that is to say, our feeling of values is not constant for a given stimulus under all conditions.

From Table XIII I have found the average displacement of each series to be as follows:

Tones, .90.
Colors, .37.
A. Touches, .28.
P. Touches, .23.

This shows that passive touches are subject to the least displacement, while active touches, colors, and tones are respectively subject to a greater variation. The sight-touch world is more stable than the auditory world. With M. the tones go over a full point both below and above the standard.

This report does not treat of the particular effects of a qualitative nature that follow from the possible combinations of series of stimuli of different feeling values, — such as the effect of an agreeable touch upon a slightly disagreeable tone, or upon an indifferent color, and so on. All such effects can be traced by rearranging the data already collected, and this may be done in a subsequent paper which may enter also into the theoretical discussion of the whole problem.

THE COMBINATION OF FEELINGS

BY C. H. JOHNSTON

THE problem at issue in the present investigation concerns the combination of feelings. On the basis of theories as different in many respects as those of Wundt, of Titchener, of Lipps, etc., the feeling-state is always a unity. The affective process for Wundt must be always "coextensive with consciousness." When he chooses to speak of a "mixed feeling," it never for him signifies a "mosaic in consciousness," but is always a new *Totalgefühl*, which "swamps consciousness as a whole."

Titchener also believes that with every affective experience an inevitable and pervasive "tilting" of the whole organism occurs. W. McDougall, in his recent Physiological Psychology, makes the general statement that always a "massive state of feeling results" when many sensations are simultaneously excited, and that in such case we cannot "introspectively distinguish the feeling-tone of each sensation."

We started to examine by experiment and by introspection which feeling-effect really results from a combination of various impressions with affective tone, and whether it is really impossible that various feelings coexist and remain distinguishable. In case they can coexist, the question arises: What mutual influences can be discovered?

For the main part of our study the simplest possible feelings were chosen, because here presumably the subjects will not be forced to grapple with complex personal psychoses, necessarily confusing from their very richness. It was thought that here they could be more nearly normal, naïve, less artificial, and able to a maximum to rid themselves of preconceived personal opinions and unaccountable associations. Here, with simplest stimulations, unsophisticated necessarily, the hope at any rate is that work with a good number of subjects of distinct emotional temperaments may bring to light certain fresh simple introspective facts, which may in their turn offer valuable considerations concerning the psychology of feeling.

Throughout the course of the experiment, except in the advanced stage when more complex states were under consideration, sounds, colors, odors, simple figures, and tactual surfaces were used. In the late stage of the investigation sentences and pictures more or less morally and æsthetically suggestive served to furnish for study the complex feeling-states.

The progress of the investigation divides itself naturally into the following four distinct parts.

I. From every experience of each individual the investigator sought to obtain from the subject's own introspection at the time as adequate a description as possible of the particular feeling provoked by the chosen stimulus. The feelings studied in this first period of the investigation are entirely those which heretofore have not been at all classified except in terms of the objects which call them forth. Part I is concerned with single stimuli affecting only one kind of sense-organ, visual, tactual, auditory, or olfactory as the case may be.

Two requests were made of each subject, viz:

(*a*) To describe as clearly as possible how the particular experience *felt*.

(*b*) To report always all the accompanying physiological or physical processes which seemed to *mean*, to result from, or apparently only accidentally to accompany the stimulus judged by him to have a feeling-tone.

The work of the experiment covers a period of two years, and fortunately several of the subjects were available for the whole period. No subject was used for more than two hours each week. In the preparatory training with sensations from only one sense-organ, the range of colors, odors, etc., was chosen as follows: Twenty colors, and as many tactual surfaces, etc., were presented in turn, and each subject was requested to make his judgment as to the relative degree of agreeableness or disagreeableness of the feelings arising in the several cases. The scale of numbers from 1 to 7 served in the traditional way to indicate approximately the hedonic value of the feeling-tones, 1 signifying highest degree of pleasure, 2 very pleasant, 3 slightly pleasant, 4 indifferent, 5 slightly unpleasant, 6 very unpleasant, and 7 the highest degree of unpleasantness. Though the personal differences were in some cases rather striking, the individual subject from day to day showed a relatively constant standard. This was done in order simply to be able to choose approximately the stimulus in the individual case likely to call up the *kind* of feeling one wished to study more in detail, and thus facilitate the progress of the investigation. In

this preliminary stage pleasant or unpleasant seemed to the subjects more or less an exhaustive account of these faint feelings. This was a means of eliminating practically indifferent shades, as there is here no special interest in the psychology of color as such.

II. Following upon this preparatory training, the second part of the experiment consisted in a similar study of the *mutual influences* of *simultaneous feelings* accompanying sensations from different sense-organs. How does the feeling of pleasure obtained from contact with a smooth surface influence the feeling occasioned by the *sight* of a pleasant or unpleasant object? Here, for example, colors were exposed in a large black frame manipulated by means of shutters easily opened or closed, at the same time that a tactual surface was being applied, or a tone from a tuning-fork was being sounded.

The introspection method was essentially the same here as in Part I.

(*a*) First, the subject was requested, without the necessary distraction of directing his attention at all to the bodily processes, to give himself up to the situation and to report as accurately as he could the kind of affective state experienced.

(*b*) Next, as in Part I again, in a repetition of the same experience, he was requested to be on the lookout for any and all accompanying bodily changes. The problem here was to discover to what extent the more complex state now in question would correspond to the specific and noticeable bodily reactions such as were noted in Part I, where single experiences presumably resulted. If different feeling-elements are in experience at once, can one fix upon correspondingly different suggested actions? Does the organism react to more than one situation, or to two sources of stimulation at once? Is affection present only when the whole organism is, to use Titchener's expression, "tilted" one way? Is the Totalgefühl the single undifferentiated result always, or can we here also detect such phenomena as summation, fusion, inhibition, and partial or total mutual reënforcement of the different feeling-components? Do the new reactions which seem to mean the feelings always refer to actions so inclusive as to result in the inhibition of any other tendencies to response, or is there sometimes a clear strife between two simultaneously conflicting feelings, two kinds of relatively self-dependent reactions both going on at once? Or again, when the hedonic or algedonic characters of two given simultaneous stimuli, such as a soft, soothing, pleasant touch with an irritating, exhilarating, invigorating but also pleasant yellow color, do not differ as to their pleasant-unpleasant character, must one be pale and empty "intel-

lectual perception" when the other is being enjoyed? These are some of the questions that suggest themselves at once.

Not at this point, however, considering the dimensionality of feeling, the four simple combinations were first studied; such, for example, as (1) a pleasant color with a pleasant touch, (2) pleasant color with unpleasant touch, (3) unpleasant color with pleasant touch, and (4) unpleasant color with unpleasant touch.

III. This part of the work was an attempt to estimate the average time-interval in which feeling-tones develop, and what influences other feelings given simultaneously or immediately beforehand have upon the time-development of the feeling-tone in question. Will certain feelings hasten and others retard a third feeling whose character remains unchanged when it crosses the limen of awareness? Does the feeling, for example, aroused by contact with a soft, soothing, yielding tactual surface, put one in such a state that he will more or less quickly obtain pleasure from the visual impression coming from a soft rich red color? What effect will a feeling already aroused by a low tone have upon the time-development of the feeling one gets from looking at a deep green color? Are there, again, pleasant feelings of certain dimensions which will be hastened by other feelings, and still others which, by the same means, will be retarded? If so, under what general principle do they seem to fall?

Here the time-development of a certain feeling-tone is taken when there are no other influencing factors. Then the comparison of this rate is made with the later reported time-interval when that feeling, again aroused, has been immediately preceded or accompanied by a feeling-tone from another source of stimulation. Here also feelings for colors presented in a frame, without any special suggestion of form in connection with them, were in like manner compared as to their time-development with the affective states arising from those same colors presented again enclosed in cardboard frames of special character. Some of these forms were very pleasing, such as upright ovals, small circles, etc., while others, frames cut purposely into irregular shapes, were to most observers decidedly unpleasant.

IV. Here complex feeling-states were in question, and evidence was sought as to how much could be detected here that would tend to substantiate or to call in question what seemed to be the fundamental principles of feeling-relations where the states are very simple. Further complications, such as three and even more stimulations at

once, were tested. After this, feelings aroused by looking at pictures of statues were studied and described as accurately as possible. "Perry Pictures" were used. *Dying Alexander*, *Venus of Milo*, the *Dying Gladiator*, the *Laocoön* group, and *Apollo Belvedere*, served to introduce sufficient variety. Then copies of these same statues were cut out from the card and presented to the subject with the same colors before studied used as background. These were allowed to play their part in the feeling aroused therefrom.

After this, pictures more or less morally as well as æsthetically suggestive were used. Millet's *Angelus* and his *Shepherdess Knitting* and Rosa Bonheur's *Horse Fair* afforded suggestion hints as to the contrasted motor significance of the complex states called forth. Here the attempt was made to find out in how far the feeling when once aroused is dependent upon the retained after-images or memory-images of the original visual stimulation, and what sort of feelings tend the longer to persist. Or again when both are taken in in quick succession, what sort of imagery and associations result. Are the resulting associations or images colored by both feeling-tones in any definite way? And if the feeling itself persists despite the loss of imagery, can it be referred merely to more internal sensations, or does there seem to be a necessity to consider it of purely central origin?

Such, in brief outline, has been the proposed method of study. In an experiment of this delicate nature there are clearly many things to guard against. There is danger that the investigator will unwittingly make suggestions to the subjects by his questions. There is a great danger of auto-suggestion on the part of the subject. The likelihood is also considerable that the subjects will fall into stereotyped forms of expression and general listlessness in introspection, where from week to week these simple experiences are being repeated for closer and closer examination. Again the special mood of the day will necessarily tend to affect all such feeling-attitudes toward slight stimulations supposed to have a feeling-tone. These and other dangers were recognized at the outset, and avoided as much as possible by such legitimate variations as could be introduced without changing the general purpose of the work. No subject was used when he felt, for whatever reason, unable to adapt himself to the conditions of the experiment. No subject knew anything of the recorded results of the others, and it was constantly urged that each person should wholly regard the present feeling in question, ignoring any remembered tone which that special stimulation had before afforded him.

It very soon became evident that the variations among individuals,

especially as to the amount of feeling and the consequent ability to fix upon the special physical processes involved, were considerable. The subjects represent types. Hence, it seems necessary at once to mention briefly some characteristics of the persons themselves who have reported these various experiences. This was kept in mind throughout, and seems of decided significance in the interpretation of the recorded results. After an examination of the results of each individual, whatever remains that is common to all will be briefly summarized.

All the subjects were graduate students in Harvard University or in Radcliffe College. Seven of the twelve had had from one to five or more years' training in laboratory investigations. Two subjects were ladies, the rest were gentlemen.

Subject A was a man of bright cheerful pleasant even temperament, responsive, very musical, alert, physically vigorous, very careful in statement, and decided as to the distinctness of his emotional states. He uses his facial muscles a good deal while conversing.

Subject B is musical, sings a good deal, is not especially demonstrative, nor always able to become adapted to the necessarily oft-repeated stimulations from the same colors and tones. This subject is especially discriminating as to shades, and has decided preferences for certain colors.

Subject C usually found it difficult to find any decided feeling-tone for many of the stimuli used. This subject is rather reserved and undemonstrative as a rule. He is not at all musical, nor does he care for art. He is a rather cool but extremely careful observer, and is always guarded in his introspection.

Subject D is impulsive habitually, flashy, responsive, especially to any suggestion of an æsthetic nature, such as forms, and very decided as to his experiences. He walks with a quick nervous step, is sprightly always, vivacious in conversation and outspoken.

Subject E is rather non-emotional as he often says. He is very energetic, full of life, quick but not precise in all his movements, always on a tension, does not enjoy without effort anything so mild as the stimulations here used, and finds introspection of this affective nature difficult.

Subject F is careful, experienced in introspective work, musical, talks a great deal, enjoys this kind of work, has decided preferences, is athletic and energetic. This subject makes use of facial, arm, and shoulder gestures quite freely in general conversation.

Subject G has a penchant for talking a great deal, is decided in his

likes and dislikes, musical, of an uneven temperament, sometimes cheerful, often cross, but always animated.

Subject H confesses he does not ever especially enjoy colors, nor respond with any sign of demonstration to any situations. He is steady, calm, apparently unruffled, and not an especially acute observer of his own states, proving in this experiment unusually undiscriminative as to simple experiences.

Subject I is rather morose, claiming to be habitually unmoved by even display of great passion or excitement. He finds it generally much easier to call up unpleasant than pleasant experiences, this being exceptional among the subjects. He is much slower than the average, and his feelings are not easily aroused. He is deliberative and confident as to his state of mind. He is nervous and often becomes fatigued before the hour's introspective work is over.

Subject J is nervous, of an uneven temperament, emotional, and quick to react to a situation of any kind, and, rather more than the others, subject to suggestion.

Subject K responds very quickly always, is habitually prompt and clear in statement, of an even temperament, and unusually interested in the experiment.

Subject L is unexperienced in this particular kind of work, but slow and careful. Though athletic, his movements are rather heavy. He is deliberate in speech and of an even, though rather undemonstrative temperament. He also is musical.

In order to verify my somewhat personal descriptions here recorded a questionnaire was given each subject to fill out according to his own personal judgment of his emotional disposition. This was done toward the close of the investigation, and the answers agree in the main with the descriptions offered above.

For the first month's preliminary practice, and with the purpose of stimulating curiosity and interest, and of testing the comparative richness of even slight feeling-experiences, a great variety of stimulations was used. Twenty different tactual surfaces from softest plush to very rough sandpaper served for the tactual impressions. Twelve different odors, as many colors differing in saturation and intensity, and tones from high and low tuning-forks, and noises variously produced, were employed as stimulations for the other senses. Besides these, circles, upright and horizontal ovals of various sizes, imperfect circles and ovals, and other irregular shapes were all presented in the same large black frame. When studied alone indifferent gray fillings were used. When complex states were in question colors

served as fillings. When the subjects thus became accustomed to these very simple but very definitely *felt* experiences, in these for the most part habitually ignored affective elements of ordinary sensations, the investigation at once became narrowed to more careful and minute attention to a few of these feeling-tones. It was soon found also that odors could not easily be used in combination, since they effectively effaced all feeling-tones for the simultaneously given colors or touches. Five colors, fairly representative for all subjects of different kinds of feeling-tones, were chosen, and were used throughout the whole investigation. These were the following: a soft deep red, light brilliant yellow, deep pure green, saturated blue, and a dingy greenish-yellow. The dimensions of the exposed surfaces were six by six inches.

For tactual impressions of approximately equal value soft plush, velvet, and two kinds of sandpaper were used, and for tones high and low tuning-forks. All the above-named forms were used in connection with the chosen colors. The subjects differed considerably as to the *amount* of feeling that could be obtained from such material. The variation of kinds and of intensity in the same subject was sometimes noticeable from day to day, but not great. It is hardly necessary to give detailed quotations from each subject. The following summary of the results of the experimental work, however, contains nothing that was not frequently reported by a majority of the subjects. This, then, does not represent at all what was once or occasionally reported by individual subjects, but what after training seemed to be reliable and definite and constant feeling-states.

Part I

Section A. The following are the collected expressions which many subjects used to describe the feeling for this *particular shade of red*. It feels as if it would be soft. It suggests warmth. The feeling is one of seriousness, pleasantness, quietness, of free repose, — a full feeling of the sense of safety. It is soothing, rich, full of strength, and inviting. One feels restful, grave, calm, appeased. There is an agreeable longing and a tendency to lose one's self in the color. The feeling is one of comfort, luxury, satisfaction, expansiveness, tranquillity, and quiescence, with no accompanying feeling of weakness by exertion of effort or energy. There is neither marked tension on the one hand, nor collapse on the other. There is a sense rather of easy self-control and command of one's body, but with no aggressive sorrow nor joy element, — a feeling of being attracted, with nothing to suggest any obstacle to the adaptation.

Occasionally to all subjects this color, and, indeed, all colors, seemed "dead," arousing no feeling whatever. Here the color "ought to be pleasant," but is only "for the time potentially not actually actively pleasant." Still more rarely did this red appear to be unpleasant. Some subjects thought that this afforded the greatest *amount of sensual pleasure.* More than any of the other colors they think it appears to "give you something." It does not so much stimulate as furnish a content itself. It has a direct effect rather than a tendency to make one wish to do something and thus give pleasure from the activity itself. Only one subject failed to find this color pleasant. His early association of it with blood and ghastly scenes could not be overcome. Some others, when a glare or glaze appeared on the red, found in it slight suggestions of stimulation and excitement, but the general decision in the great majority of cases was that the feeling was a sort of emotional massiveness compared with the effects from other colors.

In marked contrast, for the most part, appears the characteristic feeling-tone for our chosen shade of *yellow.* Almost universally subjects find such words as these descriptive of the feeling here in question. It is cheerful, brisk, pleasant[1] also, bright, gay, light, sprightly, merry, jovial, easy to get, pleasantly irritating, stimulating, stirring, spurring, thrilling, invigorating, and produces agreeable discontent. It is jolly, nice, trim, neat, awakening, full of the sense of motion, soaring, and arouses a feeling of welcome strain, of pleasure in action, of alertness and self-assertion. Here, in contrast for the most part to the red, there is no feeling of sinking into the color. The impulse rather is to be free, to enjoy motor expression, even if of some vague sort. There is a *felt* necessity to do something, a "joy of overflowing or of exuberance," it is called. There is little present here of what we mean by a suggestion of sensual richness found above in the feeling for red. Here there is less of amount of pleasure, but much more of the general activity element. Some subjects feel the demand for greater saturation, and occasionally it is unpleasant for just this reason apparently. Subjects C and B frequently reported this. They think the feeling would be more "stable" and "grave" and "secure" and "soothing" and one would not feel "unruffled," if it could be "toned down." Most of the subjects, however, think that it belongs

[1] As Mach long ago pointed out, *pleasant* is a vague term and in itself does not serve as a true descriptive term. *Pleasant* here applies to both feeling for red and for yellow, but something more is needed to distinguish these very different feelings.

to the ultimate elemental feeling for yellow that it should have just this distinguishing characteristic.

It is more difficult to describe the feeling for *green*. It is almost always agreeable. Two subjects, however, never like it. Sometimes it is somewhat soothing in character, but more often it is exciting. The feeling seems to be between that for red and that for yellow, partaking on the whole of the characters of feeling for the latter rather than the former. For all subjects associations tend to color the feeling-tone for green especially, and hence introspection for the feeling of pure color is doubly difficult. The most prominent partial feeling-tone for it is "irritating." [1] The agreeableness or disagreeableness of this stimulating character is particularly inconstant, varying greatly for the same subject, as well as for different subjects.

The feeling for the *blue* seems still more to be dependent upon the person. Many like it. Many others dislike it decidedly. When it affords a pleasant feeling, it is described in some such terms as these: The feeling is spiritual, lofty, beautiful, serene. The subject himself feels immoveable. To other subjects it is too rich and intense and painful. To one subject who heartily dislikes it always, it is offensive or revolting, calling up a feeling akin to the emotion one has toward insincerity in general. To none does this feeling seem to have any great amount of sensual significance. Even when it is called "too rich," the incongruity between the richness itself and the ultimate qualitative significance of the blue is spoken of. Even when pleasant, the feeling is of an "airy pleasure," volatile, unstable, and not reliable, nor safe and secure as is the feeling for red. One feels that it is always apt to vanish, vague, intangible, and with little immediate definiteness of meaning. Subjects often desire to call it an intellectual, æsthetic, or ideal sort of feeling.

No color was universally unpleasant. Two subjects found this *greenish-yellow* almost always mildly pleasant. For most of the subjects, however, it was unpleasant. Here were reported feelings of contraction, of withdrawal, of disgust, of doubt, of hesitation, of stimulation without definiteness, dissatisfaction, slight feeling of nausea, of seasickness, of opposition, and the general feeling of offensiveness. The necessary, unpleasant aggressiveness, unrest, or discontent characterizes this feeling. This unpleasant critical attitude where a decision is wanted but not easily gotten, is called often the feeling of uncertainty.

In no sense is this investigation a study of the psychology of color;

[1] "Irritating" as I shall use the word has no hedonic or algedonic significance.

the only purpose here is to find certain clearly defined feelings for slight stimulations, in order to find in what way they relate themselves to other similarly simple feelings from a different source of stimulation.

In a similar manner, then, the investigation was conducted in the analysis and description of feeling-tones for *tactual impressions*.

For *plush* there was a feeling of pleasure, ease, safety, and content. The mood was one of a general enjoyment of sinking one's self into the situation, an agreeable self-surrender. Here also is a feeling of unbending one's self, of general expansiveness, of relaxation. One is soothed, enjoys a suggestion of freedom from disturbance, of a "regularity" of the experience, feels at the same time strength in the suggested repose, responds to pleasant reverberating thrills by the falling off from the accustomed muscular tonicity, and hence has a decided feeling of satisfaction. To some subjects the feeling aroused by the hard, polished, glazed tin surface, possessing no "yielding" character, corresponded more nearly to the feeling for the yellow color than for the red. To all red "went best" with the plush. No tactual feelings offered such distinguishable elements for analysis, nor were they as definitely described as the visual or olfactory or auditory impressions. The sensational elements were in many cases more pronounced. The feeling for the plush, however, much like that for the red color, suggests a "settling down to," or a "dropping forward toward," rather than an aggressive "taking in" of the feeling-material.

The feeling-tone with sensations from sandpaper is grating, irritating, stirring, stimulating. The feeling is one of contraction, of withdrawal, of uneasiness. One is full of "collapsing chills," of minute little pains, and there is a decided call for an opposite kind of behavior. The sense of weakness, of waste of power and energy, of being penetrated, of strained expectation, of unwelcome tension, and of slight "wasteful excitement" results. To some subjects, notably subject E, at times the whole feeling of stimulation as such predominated, and the total effect produced was agreeable, as it "satisfied a felt need of waking up." Here again one subject, subject B, throughout the whole period of two years, failed to find any element of pleasure in any tactual sensation that was pronounced or prolonged sufficiently to furnish material for introspection.

As regards simple *tones* from tuning-forks the subjects find little to say. All are pleasant, as a rule, and almost universally, *low tones* are most pleasant, richer in content, greater in amount of "general appeal," more soothing, and pleasantly stimulating. The feeling of

the easy attitude called for contributes to the whole feeling. *High tones*, calling for more activity on the part of the subject, more strain, and greater stimulation, coupled with some rather unprepared-for irritating elements, are less pleasant, and also more limited in their general appeal to the whole organism. The noises variously produced were at first unpleasant, and the only assignable reason seemed to be that their suddenness came as a shock. If expected or continued they too became pleasant very often.

Feelings for *forms* seem to relate even more definitely to the activity element. The pleasure for the most part is described as being far less sensual, if indeed, so at all. Small *upright ovals*, $1\frac{1}{2} \times 1$ in., are most pleasant, because somehow they are "more suggestive of definiteness." *Circles* one inch in diameter are next in order of value as to their feeling-tones. *Horizontal ovals* are less pleasant still, though for most subjects not unpleasant. Upright ovals are best, as the kind of action apparently called for by the aroused feeling is most agreeable and suitable to the subject's natural upright position of body. An explanation of this general result of introspection, as well as the preference for the particular size chosen almost without exception, is attempted in another part of this report, where are given in more detail the various kinds of bodily accompaniments. The feelings for those ovals have also the characters of stimulation, mild excitement, and a feeling of easy freedom in a pleasing kind of activity. Tension is always present as an agreeable element when reported at all. This element is coupled with the "feeling of assurance of certainty" which the whole situation calls for. It often seems clearly to suggest that one do something. Circles tend more to suggest inner stability and completeness. They stand on their own axes. Here there is a sense of satisfaction, complacency, and sufficiency. The feeling here of a call for immediate activity on the part of the subject is weak and indefinite, when not altogether absent. The subjects do not use for this experience such expressions as excitement, tension, irritation, quick contraction, or the impulse to self-assertion. Horizontal ovals are least pleasant, it seems to me, for obvious reasons. Here such noted elements as "felt unnaturalness," "difficulty of adapting one's self," "wrong direction of activity," which alone and in themselves would be unpleasant, are nevertheless more than counterbalanced by other and pleasing elements, such as symmetry, definiteness, partial stability, and other agreeable features. Often these latter features are not pronounced, and then the judgment is, that the total feeling is unpleasant.

Likewise as regards so-called bad forms, no single statement is

unqualifiedly true of any considerable number of subjects. The decided feeling of irregularity, the "bulging-out" or the undesirable "pushing-in" of the figure, the feeling of weakness in one's own body corresponding, the feeling of instability which one tends himself to imitate in various ways, the total effect of lack of poise, all tend to make these figures on the whole unpleasant. But one cannot even here count upon the constancy of the subjects' feelings. At times, due perhaps to undercurrents of association processes of which even the subject himself is not clearly aware, the figure suddenly looms up as quite definitely pleasing, and full of vague suggestiveness and hidden richness of content. These varying characters of the feelings for forms come out interestingly later in the study of them when they are presented as frames for the above described colors.

Section B. The bodily processes noted by the subjects are numerous, and here also, just as with the amount of feeling above, the personal differences are striking. Some subjects detect a great many forms of organic commotion, others rarely find anything that can be said to be descriptive or explanatory of the feeling-state. To all of them at first this looking for bodily accompaniments destroyed the feeling itself. Only after considerable training was it possible for them to find any physiological processes that seemed at all significant. As a general statement the evidence would all tend to suggest that feelings for color are most readily and directly referred to the head, face, throat, and particularly to the forehead and to the eye-muscles. When, however, the feelings are particularly strong, they tend to pervade the whole organism. Red thus often brings about the suggestion of general bodily comfort, and yellow, when very strong, arouses the impulses calling for "spreading-out, aggressive movements," referred to arms, shoulders, and chest. Tones have in general the same reference to the head. Odors are always more organic, affecting more directly the respiration, muscles of the abdomen, and the more internal apparatus generally. Tactual impressions refer to the trunk rather than to the face, hand, arms, or legs. Forms seem to call forth imitative movements, and the actual or incipient motor impulses refer to the action of the eyes in motion, the position of the head, of the whole body, of the shoulders particularly, of the shaping of the cheeks, lips, etc., and of the similarly imitative actions in the hands and arms. The following is a list collected from the reported bodily references given for the feelings described in Part I, Section A.

(1) Free full respiration and free activity of all voluntary muscles;

or, for other feelings, the checking of respiration and often the lack of impulse to move at all, with no suggestion, however, in most cases, of lassitude.

(2) Chest expansion and general relief pervading the whole body. The expansion or contraction is further modified by the *degree* of *regularity* and by the *rate* of the movements involved, as also by the *ease* or *difficulty* in the performance. So also, in the cases of feeling whose tone exists but is doubtful in character, the bodily situation seems to mean "lack of movement or change in any definite direction." The feeling-tone and its vividness are interdependent and reported as closely connected.

(3) A cringing all over and a "holding up of all activities."

(4) Abdomen contraction, chest and shoulders drawn in, hands clenched, and jaws set.

(5) A feeling at once in different parts of the body of both process of contraction and expansion.

(6) An incipient feeling of nausea in the digestive tract.

(7) A tendency to incline the head forward or backward, or to keep it rigid, or to turn it aside.

(8) For touch, waves, reverberations, pleasant penetrating thrills in the chest and abdomen especially, less frequently in the limbs, occur. Sometimes these suggest expansion of the whole frame; sometimes, even when also pleasant, the tendency to contraction and tension is noticeable, but in these latter cases the contraction seems to be rather definitely the calling into action of those general innervated muscles which refer to the bodily situation of one when he intends to go toward the pleasantly stimulating object.

(9) For unpleasant touch the reference or localization of the bodily response is, when reported definitely at all, generally in the back, described as chills not thrills, contractions always, contractions also which often suggest shivers of withdrawal. These feelings also are referred to the situation of the trunk of the body, and are felt to originate in the small of the back, and in the back of the shoulders. For two subjects there occur twitchings in the tendons of the hips and thighs, and movements of the knee-cap.

(10) The pervasive bodily collapse, which seems to accompany feelings characterized as depressing, altogether unlike the soothing feeling of unwearied repose given by certain soft rich colors or by low deep full tones or smooth yielding surfaces, is another form of organic response which is often spoken of.

(11) The direction of the stimulus with respect to the normal posi-

tion of the body also seems to have something to do with the regularity of the response, and with the general forward or backward tendency. Tactual surfaces applied or tones sounded behind the subject do seem to make the bodily adjustment more confused, and less pleasant. All that subjects could say was that the position was felt as abnormal and correspondingly less pleasing.

(12) In many unpleasant feelings, where there was no specific localization possible, the "stiffening tendency of hardening one's self to a necessary experience" was frequently reported. In the case of other states of undifferentiated pleasure a "consenting bending forward of the whole body" was often detected.

(13) Many stimulations seem to demand that one draw one's self erect, square the shoulders, and "assume the attitude of alertness."

(14) Certain colors for almost every subject independently hint at sea-sickness. Others, as noted above, report the incipient suggestion of nausea in the digestive tract. Indeed, abdominal references are frequently reported by most of the subjects. The abdominal muscles become "eased up," or again there is a "sucking-in of the belly."

(15) The feeling of "being natural," of regularity, a universally popular feeling, is described as a pleasant relief from all tensions and habitual inhibitions, or a dropping of one's characteristic muscular tonicity.

(16) Other stimulations still, particularly certain delicate odors, for men, subjects C and E for example, seem to suggest what they call the "childish play impulse." They are called "simple, foolish, childish pleasures," ignored in ordinary life. They are slightly pleasantly irritating, and merely make one wish to do something. It is pure bodily restlessness, a general kinæsthetic enjoyment. Three subjects, especially, find here the frequent twitchings in the calves of the legs, in the knee-cap, and the more decided innervations which contract the tendons of the thighs and hips.

(17) Subject I frequently detected sensations of contraction in the tensor tympani connected with the pleasure derived from high tones. Others referred feelings for tones partly to the regions of the ears.

(18) The kinds of facial references are numerous. General contraction or expansion around the eyes, forehead, temples, sometimes to the whole head, and quite frequently it seemed as if the feeling referred to the very inside of the eyeball, to the iris and accommodation movements.

(19) Subjects A, D, F, and K noted specific incipient tendencies to

smile, to smooth the brow, and to "unbend the face" as characteristic descriptions of certain oft-repeated experiences.

(20) Introspections from subjects F and G quite constantly revealed articulatory impulses vividly accompanying the feelings for many colors and forms.

(21) A scowl and puckering of the lips was descriptive of the attitude taken toward some unpleasant situations.

(22) A contraction or relaxation of the throat-muscles and of the vocal chords generally was not infrequently noticed. The tendency to swallow is spoken of. The throat is felt often to be "concave" when certain bad feelings are sufficiently pronounced. A contraction in the mucous membrane, with teeth on edge, such as one would experience in eating something sour, is frequent. A twitching of the ears, squinting of the eyebrows, and a "heavy feeling" through the neck and chest occur often, or again a pressing hard of the tongue against the roof of the mouth.

(23) Forms suggested a shrinking in the volume of the face, sometimes of the crown of the head, and even of the whole head.

(24) Upright or horizontal ovals especially provoked the impulse to imitate the figure itself, either with the lips or with the hands and arms. When the feeling was particularly strong, all these impulses often occurred together and appeared mutually to reënforce, or to intensify each other.

(25) Horizontal ovals gave one the feeling of being "flattened out," coupled with an impulse to adjustment altogether unlike the sprightly, alert, airy feeling aroused by the "trim," upright figures.

(26) Occasionally when the irregular shapes were presented directly after a subject had been enjoying one of the perfect figures, that side of his face or body corresponding to the distorted portion of the figure was felt to be in an abnormal and unpleasant position. This "caving-in" or "bulging-out" sensation, which accompanies the unpleasant feeling, happens when the whole muscular system at the time for the subject seems inert or externally controlled.

All these sensations of bodily processes, taken from the introspective descriptions given by the subjects, are distinctly reported by them as very faint. They by no means detect them in every experience, nor do they always seem to the subject himself to *mean* the whole of the feeling as experienced. Neither did any one subject find all the concomitant processes recorded above. Subject H failed throughout the whole period to detect anything whatsoever, except slight tendencies to frown, smooth the brow, or to open wide the eyes. This subject was

unable to detect any special differences in his feelings, either in variety or in amount. For him neither soft red nor brilliant yellow was either exciting or soothing. They were and always remained for him more or less vaguely pleasant, and this description for him was both ultimate and exhaustive.

Subject B could get no kind of pleasant feeling from any tactual surface, while to Subject E even the coarsest sandpaper usually afforded pleasant stimulation. As spoken of above, articulatory impulses were characteristic of the motor tendencies of Subjects D and G. To Subject A the experiences seemed richest and fullest, and the corresponding bodily processes were likewise more pronounced and varied. In the great majority of the experiments, especially during the period of training, the feeling itself vanished when the subjects attempted to analyze the bodily processes. It was chiefly, however, a matter of training, and this more and more ceased to be a disturbing element.

Some subjects preferred often to speak of circulatory, or at least, decidedly internal and usually involuntary changes in addition to, and sometimes without, the controlled muscular actions. The mood of the time affects the amount of feeling, and occasionally, but far less frequently, the quality. The moral significance of the feelings was most prominent when the subject felt most interested in the experiment, as may be noted above in their descriptions of the feelings for red and yellow. What may be termed the "*regularity element*" would seem generally to serve as the test especially for the pleasant-unpleasant character of the feeling-tone. The feeling of expansiveness never accompanied unpleasant feelings. Feelings of contraction, on the other hand, very often occurred when the feelings were not at all disagreeable. In such cases there was a significance attached to the *direction* or meaning of the adjustment.

Part II

Section A. Here simultaneous stimulations of different sense-organs were given, and the situation became at once more complex. For some time only colors and tactual surfaces were employed. Later tones from tuning-forks and noises were added. Forms with different colors as fillings still further complicated the experience. Odors as a rule were unsatisfactory, being so strong as entirely to inhibit all noticeable effects from the other senses.

For all subjects at first the feeling-tone related only to the one object

directly attended to. Some effort is required to detect the feeling-tone for these slight stimulations, and while this is being done, the feeling for the other sensation tends to vanish. If, while enjoying the soothing contact with the plush, a chosen color is disclosed in the frame and attended to sufficiently to obtain from it a decided feeling, there is a distinct awareness of the dropping of the feeling for the touch. To some subjects, whatever the combinations used, this almost constantly occurred for perhaps a month. Often again there seemed to result a total "feeling of the situation," when the attention was on neither stimulating object.

Frequently, too, the attempted introspection at this point failed to fix upon any feeling-tone at all definite. The condition was one of confusion and bewilderment. The state of mind when one cannot feel at all definitely seems to correspond closely to that state of mental confusion when thought processes are in a jumble, with no path for the moment leading anywhere. All these difficulties were overcome, partially at any rate, by continued training. It was not as if introspection revealed the fact that there was nothing to be found, and this was frequently reported by the subjects. After some time the touch character could be retained, and its peculiar value for feeling did not disappear when other things came in and contributed an affective element of their own. The old law of the opposition, or mutual exclusiveness, of feelings would thus seem to mean little more than that we, generally speaking, experience one thing at a time. Without a special analytical purpose in view, we do not find many distinct elemental feelings, as we do not, until we psychologize, find elements of cognitive character separable. It has been, and is now, commonly supposed that myriads of ideational elements, partially analyzable at any rate, go to make up what we choose to call a single perception. This experience as a whole is of some affective nature; but, as generally stated, of one unanalyzable sort always. It is true just in the same sense as in the cognitive state, perception. In the sense that every perception is unique, in this sense every affective state is likewise a unit. The evidence I submit, however, is that one may be the subject of analysis into elemental parts just as much as the other. Affection, as Titchener defines it at the beginning of his treatment of feelings, is merely a "tilt of the whole organism." If this is the ultimate statement, then there are no combinations, and no relations of feelings except that of mutual exclusion from the field of awareness. He has taken only one of the above possible attitudes toward affective states. Geiger, in his study of very complex emotions, however, has taken the other attitude, and

bases his whole position upon it. This present experimental test furnishes evidence that the latter position is also a legitimate, and perhaps more desirable position, if feeling shall have scientific analytical treatment.

In this investigation, after considerable training, the subjects, with a single exception, *were all convinced that both feeling-tones, for tactual and visual impressions, could be present at once.* When three or more were given at once, confusion as to the state of feeling was usually so great that valuable introspection was always rendered exceedingly difficult. Impressions from the same field, as, for example, colors presented in chosen forms as enclosures, were most often taken as one object with one feeling-tone. This even was by no means always the case. When it was thus taken, the experience was still reported as more complex than either element alone had produced.

When the feeling-tones for simultaneous stimulations from two different sources came out sufficiently clearly, the kind of feeling was described in some such terms as indicated in Part I, Section A, except that almost invariably the introspection was more difficult. The relations of these various feeling-components of an affective experience are numerous. There is a frequent tendency to read one into the other. The soft soothing feeling coming from plush, if in the particular experience the color be the more prominent partial element, tends often to make the subject enjoy more the color, because there seems to be added to it a soft yielding surface texture. Frequently also, as in the case of red above, the warmth it suggests is intensified.

In cases of feelings of opposite nature occurring together, the stronger generally prevails, finally in most cases effacing all specific tone for the weaker element. An odor, for example, even when always unpleasant, becomes less so when one looks at a pleasant color, when a feeling-tone can, or often even when it cannot, be detected for the color at the time. Again, when a very unpleasant form or tactual impression is being felt, a slightly unpleasant color tends to arouse often in this situation, as if by contrast, a simultaneously pleasant element in the total experience.

For many subjects frequently there results what I shall call a "Total Mood." This, as to its feeling-character, can be merely different from, more than, less than, or the same as either component or of both together. To some the feeling is proportionate to the degree of concentration of attention, and in all such cases rarely does the whole complex situation afford a feeling equal to that given by either component alone, the extra stimulation for the time being simply a disturbing factor. To

others the shifting of the focus of attention from one to another of the external objects of interest, or from one feeling-element to the other, is not at all disturbing, "any more than is any general state of satisfied self-contemplation." This kind of experience is often and distinctly reported, not as the enjoyment of two where the discernible elements persist wholly unrelated, but rather an enjoyment (or disagreeable experience as the case may be), simply from two sources of stimulation, a total mood with similar or harmonious constituents. The red color and the tactual feeling for plush afford this. Similarly the unpleasant color above combines with certain odors or with the sandpaper. Yellow, however, does not as a rule produce a feeling that peaceably "falls in with" the tactual impression brought about by the plush. Low tones tend to combine thus with the red color or with the softest plush in the same kind of Total Mood. The feeling-tones usually for pleasant high tones are described as "falling in with" the feeling for yellow when the feeling exists as described above, and as nearer to that of the feeling for the green color than for the particular deep shade of red. What may be termed the "*Congruity* or *Incongruity of Feeling-Tones*" is perhaps a good name to designate feeling-tone relations. It implies neither mutual exclusiveness nor total fusion, and some such term is necessary.

The various phenomena of fusion, summation, partial reënforcement, merely simultaneous, independent coexistence, partial and total inhibition, of one by the other occur. The feeling-tone for yellow tends most readily to fuse with the feeling-tone for high tones and upright ovals. This is not so marked for the green, but more so for all other colors than for the red. Red harmonizes and tends to fuse, for most subjects, with the feeling-tones for soft plush, low tones, and circular forms. This harmonizing, however, is not all that contributes to the amount of feeling in these complex cases. Subjects often prefer the low tones with yellow, even though there is less harmony. So also upright ovals are in themselves generally so much more pleasant than the circles that red is preferred thus presented, though its feeling-character is more akin to that suggested by the circles. These are cases where the intensity itself of the feeling-tone is preferred, even though what is felt to be an harmonious combination is lessened.

When the situation admits of a complete fusion, the one resulting feeling is almost always greater. When summation of unpleasant stimuli occurs, the singleness of the attention process is not a prominent feature of the experience. Rather each unpleasant element exists throughout, each in turn intensifying the whole undertone of feeling,

but also remaining a feeling-tone of a particular kind. Partial reënforcement is descriptive of that state when both feeling-tones contribute to a feeling of the same kind, yet do retain some individual characteristics which stand out for themselves. The general state of pleasantness, for example, is increased by both elements contributed by a low tone and the yellow color, yet one retains its soothing and the other its exciting character. Again, the feeling-tone for green may occur when its relation, on the other hand, to a pleasantly sounding tuning-fork is not at all noticed. Subjects find in such cases always more effort required to note both the feeling-tones, and there is probably some diminution in quantity of feeling for each of the simultaneous elements. Other subjects have preferred to call this partial inhibition. Cases of total inhibition have been noted above, and are by far the most frequent, as would naturally be expected. When sandpaper is being applied, and no repose is felt in the body, a color, suddenly presented, for a moment pleases the eye, but quickly loses all feeling-character, and can only be "intellectually perceived."

Again, the way in which subjects will take certain combinations seems to depend entirely upon the person. Beautiful colors, presented in disagreeable forms, bring about for some a feeling altogether worse than does an unpleasant color in the same form. To others there is always the tendency to enjoy the color and to "reconstruct" the form, or stress in it those elements only which do suggest symmetry and definiteness. All feel, when two or more elements contribute to the feeling-experience, that a total mood generally serves as the undertone for them. When there is a clear strife between the two, they both can exist as equal partial tones with an undertone of unpleasantness in the failure to coördinate them. There are still other cases where the total result cannot well be called a fusion or summation. For example, when an unpleasant color in an unpleasant form, or for Subject D, a pleasant color in an unpleasant form, is presented, the feeling for the whole is often out of all proportion to the value of each alone, or of what might be expected from the simple summation. The uncommon revulsion here was frequently so striking that the subjects would afterwards laugh heartily over the strength with which it first appeared.

Section B. Introspection here as to the physiological accompaniments referring exclusively to one of the two or more existing feeling-tones is still more meagre, but at times very definite. When the elements of a total feeling fuse there is of course no reference to the

particular processes which bring this about. It is then simply a general response to a situation. *When, however, distinct, or opposing feeling-tones are present and detected, they do often mean opposing inclinations to action.* The yellow color can retain its exciting tone, and refer clearly to such activities as opening wide the eyes, incipient smiling tendencies, and general alertness of facial expression, when a soothing touch is also felt as suggesting a toning-down of the body and a general relaxation of the muscles of the abdomen. This is the most frequently noticed effect. Tactual impressions are accompanied by pervading organic feelings in the trunk, while visual and auditory stimulations, in their incipient stages, at least, have the more pronounced effect upon facial muscles of expression, and general sensations in the head. When any of these feelings are particularly strong, however, the sensations, whose feeling-tones seem to constitute the feeling in question, tend to pervade the entire system and to usurp the whole bodily activity. The motor tendencies noticed above for the irregular forms are also reported when the color itself remains pleasant. Yellow, possessing more of this activity itself, is least pleasant when exposed in these forms. The opposition of tendencies is noticed, yellow meaning its own peculiar kind of aggressive movement, and the bad form at the same time calling for that irregular kind of unpleasant adjustment. Red does not "intrude itself" nor demand action, and is always less strikingly in opposition to the form than is the case with yellow or green or blue. Forms, almost perfect, relate themselves to feelings of tension. One feels that he cannot quite take them as perfect figures, and this strain and inability to take them for what they suggest provokes a decidedly unpleasant feeling. Very irregular forms become "grotesque" or ludicrous, and the bodily change is indicated as a "jumble of partially carried out reactions."

In many cases sensations or motor tendencies are noted all over the body during the existence of these complex states. At such times they are not recognized as referring to either feeling-tone in particular. When also a favorite color is presented to a subject who is experiencing a disagreeable feeling from sandpaper, the touch is so pervasive usually that he feels that this "controls the whole response" and inhibits any reaction, or even any suggested reaction to the color. When there does fail to be even any possible incipient motor suggestion, as a rule the feeling-tone for the object is extremely vague if it exists at all, and the object appears for the time "dead" or "valueless." Subjects speak of their own inability to respond in such cases. It is not at all as if the color is definitely bad, but rather as if one cannot

do two different things with the same muscular apparatus at once. As often, as has before been noted, does the opposite occur. Colors in definitely characterized forms illustrate the relations of similar activities when feeling-tones occur together. Yellow is preferred in upright ovals, for both accentuate the same demand for activity, and calling for the same kind of response, tend to fuse into a single object. Yellow and plush do not harmonize, and in many cases where both retain their feeling-tones, distinct activities in different parts of the body are aroused simultaneously. With the circles the feeling-tone for yellow does not agree with that for form. The yellow becomes almost unpleasant at times. Circles are "heavy," "stable," "on their own axes." A yellow thus enclosed seems "too fat," too "unnaturally heavy," not free and light, and the effect is less pleasing. Circles suit the red better than they do the yellow or the green or the blue, and tend to be seen as one object, or to fuse, more readily than red in an upright oval form. The feeling-tones for red and for upright ovals are both very pleasant, but not as much in harmony, and consequently usually taken as two different feelings.

As a general result of introspective analysis at this point, when different feeling-tones did occur together, they were described in terms similar to those used when each alone was experienced. The bodily references, when found, were of the same character, the only difference being that there was much difficulty in determining to which feeling-tone the response referred. In many instances, however, again it seemed quite certain that different kinds of adaptation in different parts of the body were suggested which seemed to correspond to distinct affective qualities. Also distinct feeling-tones, each of which alone could call forth a similar kind of action, when given together tended to accentuate the total unified response. The upright ovals mean alertness and soaring motions with a general suggestion of drawing the shoulders up. The yellow color accentuated this. The circle with the soothing red, or the fusion of feelings for red and plush, pleases in quite another fashion.

Part III

In attempting to measure the rate at which feeling-tones for those slight stimulations develop when no disturbing factors are consciously present, an interval of from one and one half to two and one half seconds seemed to be required. At such a time the feeling was experienced as having reached its maximum. There was no marked difference

for different subjects, nor any constantly noticeable difference among the kind of stimulations used. A possible exception was found for Subject I, but this was probably due, as he himself thought, to his inability to adapt himself easily to the requirement of the experiment.

After this was sufficiently tested, the interval which was required for one feeling-tone to arise when another was already present, was in the same manner tested. The interval in all cases was too long to be measured by means of a chronometer. A stop-watch was used.

While the subject was consciously enjoying a sound from a tuning-fork or a tactual impression from some chosen texture surface, one of the colors was presented to him. The time-interval thus ascertained as necessary for the new feeling-tone to reach its maximum was compared in each case with the time-interval when the color alone was presented. Various combinations were here employed also. Colors in forms in addition were studied in comparison with the same colors presented without regard to the enclosing forms. No definite results could be obtained in most cases. It was thought that the repose one feels for plush might appreciably hasten the feeling-tone for the red and probably retard that for the more exciting yellow. The evidence is not directly conclusive. This was not found to be the case in much more than half of the tests. It did, however, in the great majority of the cases with all subjects, retard the time-interval for the development of the unpleasant character of ordinarily disagreeable colors. Given at such times also the normally unpleasant colors not infrequently appeared themselves as slightly agreeable. In these cases the interval was also appreciably longer, suggesting evidence that *new* processes of some sort were set up. A pleasant low tone hastened the arousal of a pleasant feeling-tone for red quite perceptibly for three subjects, and had no influence upon the other subjects. The feeling-tone for yellow under the same circumstances was for two subjects retarded regularly, with no marked effect either way for the others. The same low tone retarded all the unpleasant colors, as did the plush, in many cases causing them to appear as pleasant.

The effect of forms, as enclosures for colors, upon the time-rate was more marked and constant. Subject I again was always disturbed when colors were presented to him in definite forms. For him feeling-tones never arose so quickly when the form-element entered. For the other six subjects, available for this part of the work, upright ovals considerably increased the whole state of pleasure whether or not fusion of the different elements resulted. For them the feeling-tone for every pleasing color was hastened from two fifths to four fifths of

a second. These same forms retarded the unpleasant colors whenever one element of the experience seemed to be opposed to the other. Occasionally here also the color appeared as itself directly and unaccountably pleasant, the prepared situation of the subject being such, apparently, that the ordinary character of the color did not appear at all. This was very frequently the case for all subjects.

The irregular unpleasant forms generally retarded the feeling-tone for the enclosed color when that color appeared to have lost some of its accustomed agreeableness. When, however, the contrast in feeling-character between the form-element and the color-element as such was noted as marked, the feeling-character of the color was more often hastened than retarded. These same forms in almost every case (of nearly two months' work for seven subjects) hastened the feeling-tone for the corresponding disagreeable color. Often again pleasant colors changed the feeling-tones for these irregular forms. In such cases the influence could not be attributed to the effect of unpleasant forms upon feeling-tones.

Statistics alone seem insignificant here. Each variety of affective experience in itself presents its own peculiar difficulties. In a great number of tests the affective phases of the experiences were all described in such terms as to suggest that too general a grouping of them would not mean much. Often when one thought, after a careful choice of the stimuli to be used, that the experiment would show that feelings whose prominent characteristics were those of excitement or tension, for example, were exerting an influence upon some other kind of feeling, introspection would reveal the fact that altogether other phases of the experience were the pronounced elements. Examples of what at first appeared to be capricious results illustrate the baffling nature of the problem here dealt with. Red is very pleasant. The oval with the bulging side is repulsive. This combination caused no marked retardation in the time required for a feeling-tone to develop. The blue, not so markedly pleasant alone, with the same bulging oval as its frame, had its feeling-tone changed, and the time-development quite perceptibly hastened. This same blue color with an upright oval as its frame produced a feeling-tone much more pleasant, also with marked hastening of the speed-development of its feeling-character. The pleasant-unpleasant dimension of the feeling clearly cannot alone furnish one with an explanation of these different phenomena. The red under normal conditions, *i. e.*, if not influenced by either favorable or unfavorable coexisting feeling-tones, aroused its peculiar and not necessarily pervasive kind of physiological process.

Likewise all our evidence goes to show that the feeling for blue is correlated with a peculiar physiological process, not so deeply seated in the organism, and not so satisfactorily coördinated, or "definite." Now the specific feeling-tone for forms arises when the imitative adjustment called for is successfully accomplished. In the first combination cited above the feeling-tone for red, being mild, soothing, more pervasive than blue, but lacking in the exciting character, is correlated with processes not so easily influenced by the reactions occasioned by the presented forms. Subjects say that it does not call for "surface reactions." It is less "intrusive." It does not "fall in with," nor does it strikingly oppose, the necessary reaction to the forms. Its influence upon the time-development of feeling-tones for accompanying stimuli is consequently small. This is not the case with the blue. The explanation, however, does not here differ in principle. This "volatile, unstable, indecisive, thin, or shallow" feeling, can be more easily influenced by the definite and decisive processes characteristic of the forms. It, indeed, needs something to determine its character, or coördinate its general reaction. Hence in both the above combinations the development period of the new feeling-tone for blue is shortened. The feeling reaches its maximum in either combination more quickly than when it occurs alone. As one should expect, fusion or mutual reënforcement quickens coördinated reaction; and partially independent coexistence, except where the contrast is sharp, serves as a condition for the lengthening of the latent period of feelings.

Part IV

It is beyond the province of this paper to report the accounts the subjects have attempted to give of the complex feelings aroused by the pictures of statues. The primary and limited purpose is to try to trace out the influences of the feelings before dealt with for colors when these are also present in some way related to the now complex æsthetic states. The *Einfühlung*, often reported for the simple forms, is here much more easily detected, if the statues arouse agreeable feelings. They "work themselves into the statue," or assume the position, or the facial expression if this is prominent, or feel very strongly in their own body what seems to be the most prominent element in the feeling portrayed by the figure. Few subjects liked all the statues. Incipient if not actual tendencies to motion of some sort, with the sensory counterparts to these situations called for when the subject feels that he is in the "proper attitude" to get most feeling from the presentations, chiefly constitute what was in different ways reported.

These statues presented on colors as backgrounds are variously and interestingly modified. The feeling-tones for colors distinctly affect the meaning of the statues. Of the above colors our shade of red is preferred with Venus by all subjects. Here the feeling-tones more nearly fuse. Always the feeling-character of the statue predominates, and the other feeling-elements of the situation are accepted or rejected in proportion as they harmonize or fail to harmonize with the predominant partial tone. Red, for example, here adds to the "richness and luxuriousness." It accentuates the strength, poise, grace, balance, ease, rest, wisdom, composure, endurance, and dignity. It is more soothing, and calls for no unnecessary action. One subject never liked any color as a background. In this case colors were good in proportion as "they kept out of the way." This is the reason for red being always preferred to yellow or green. With these latter colors there is an interplay of reactions not coöperating. The color-exciting element is more immediate, tension is brought about, the color asserts itself, is pleasant, and tends directly to inhibit the feelings for the statue. The pleasure in the color is called "thin" in comparison, and the power of sympathetic appreciation of Venus is lessened. There is suggestion now in the statue still of its strength, but with no "enduring" quality. It has become commonplace, merely a "pretty woman," jaunty, self-sufficient, cynical, and with little dignity. The motion element, now prominent, is not pertinent. The statue looks "cheap," and as a figure is volatile and unsteady.

In a similar way one finds these feeling-tones for colors variously playing their part. The statue of Apollo is not pleasing to some subjects. They want it "toned down." The red effects this. To some it is most pleasing by its suggestion of easy grace and springy, elastic step. The yellow harmonizes and accentuates this chosen feeling. The blue often destroys its moral meaning. The red hampers the feeling for the *Laocoön* group. They become listless, dead, and have still strength, but no struggle. Yellow increases the amount of activity, but often lessens the "serious despair." Fierceness is added, but the liveliness thus furnished is at the expense of the necessary balancing solemnity. The color again becomes intrusive. "The snakes fairly dance," and the "flashing action behind" the statue is now too prominent.

For the *Dying Gladiator* or *Dying Alexander* red is preferred. It, however, as do all the other colors, often produces an overbalance in the whole situation. Here it suggests no conflicting feelings. It adds — often too much — to the hopelessness of the situation, and gives to

them an exaggerated solemnity and resignation, which emphasizes a melancholy cast, not altogether called for. Green and yellow are always incongruous. They tend to distract the attention to certain particular muscles, thereby lessening the whole general effect. The little "prettiness" they still retain is not called for, and is not "of the right sort." They do not allow one to be sufficiently contemplative or thoughtful. They have little depth, and cause inharmonious bodily commotions, and too much intensify the life-struggle and anguish.

The general effect of the statues here is much like that of the simple forms above. Both not only call for something to be done by the subject, but some action more or less already definitely outlined. The Einfühlung for little wooden figures, such as cones, columns, pyramids, etc., was clear and decided. The tipping character or the straight erectness caused a feeling which seemed describable in terms of the way in which the bodily position, as one naturally adapted himself to the object, took place. Statues afford richer experiences, but the principle is not different. They seem full of suggestions of abstractions, such as strength, wisdom, grace, beauty, power, and, in general, what are often called spiritual feelings. These are not so easily imitated in detail. Subjects have their own ways of adapting themselves. They want to carry out the suggestion or impulse in their own way. These impulses are projected into the figure, and all of the vigor inhibited in one's own body becomes a living part of the figure. The impulses thus from the colors may or may not be of such a character as to bring about the same proportionate adjustment as a desirable intensification equally of all the feeling-elements. Whether desirable or not, however, these feeling-combinations furnish additional illustrations of the various mutual relations of coexisting feelings.

The *Angelus* or the *Shepherdess Knitting* bring about feelings in striking contrast to the feeling for the *Horse Fair* picture. The latter arouses suggestions of a tumultuous bodily condition, increased muscular tonicity, muscles twitching everywhere, breathing heavier, shoulders strained, and in comparison, great general innervation. When the characteristic feeling had been aroused, the subjects were requested to close their eyes and observe if possible to what extent the feeling already aroused was dependent upon the retained images. The results were clear. If the feeling is slight, as it is for some subjects, the feeling tended to vanish and return with the recurring images. If it is fairly strong, the feeling persists for some time after visual imagery is lost. If very strong, the feeling is constant for a still longer period and still less dependent upon the original peripheral

excitation. The feeling is always more constant than any imagery. Not often for example is the whole picture retained. Sometimes one prominent part only remains. Often again various kinds of imagery aid in preserving the feeling. Besides visual, auditory, the sounds of the horses' hoofs, of the tones of the Angelus bell, are chiefly prominent in preserving the situation and the condition for the feeling. Articulatory impulses again, in the tendency to repeat to one's self such words as are descriptive of the moral meaning of the pictures, offer sufficient clues to keep the desired feeling aroused. When the feeling has "struck deep," subjects report motor imagery pervading the whole system. In such cases the recurring visual imagery has little effect upon the feeling. On the whole, the feelings for the more quiet pictures last longer and are more easily retained than is the case with the more exciting ones, if the original feelings are, as to mere intensity, approximately equal.

The character and strength of these feeling-tones determine also to a large extent the lines of association followed. Here the mutual influences of feelings are clearly recognized. The character of the new associated images and situations is colored by the feelings which were connected with the original stimulations. The pictures, such, for example, as the *Angelus* and the *Horse Fair*, were presented to the subjects in quick succession. These were to be merely starting-points for association. For all the subjects who were able to report anything definite, the feeling-tone for one was read into the associations which were aroused by the other. The second of the two starting-points as a rule controls the imagery. A few examples will illustrate how both feeling-tones are retained. The *Angelus* was presented first in these cases, and the *Horse Fair* second.

For subject K, the parts of the picture of the *Horse Fair* remained. The feeling of seriousness and quietness, foreign to it itself, was projected into it. Solemnity and the feeling of strength and power was accentuated. The gaiety originally present was very much lessened, and finally not noticed at all.

For subject C, a sacred feeling was aroused. Wars of the Bible were recalled. There was a fusion of the imagery. He saw the church on a battlefield near a cavalry fight. The feeling of active earnestness and the sacred moral character was reported as due to the retained feeling first brought about by the *Angelus*. The influence of the other starting-point is clear.

Subject B found the incongruity between the two feelings very strong. The *Angelus* was the stronger in influence. The other caused

one to stress the lighter, more trivial character of the former. Meadows, streams, pools, and enchanted regions typified the fanciful mood thus brought about.

It is not a question as to whether such trains of thought would have occurred if only one starting-point had been used. It is rather that, in such cases as the above, two distinct feeling-tones were actually detected as playing their part in the resulting complex experiences. It is with some effort that both feeling-tones can be thus at first retained. The resulting undertone or general mood, however brought about, colors and determines to a certain extent the associations which follow. The feeling is more deeply seated than the image, and here also it is retained longer.

The above recorded account of the behavior of simple feelings fairly represents the accumulated data at our disposal. How they can be adjusted to modern theories of the relation of consciousness to movement may be briefly suggested. Yet the rudimentary state both of the psychology and of the physiology of feeling makes the present task a hazardous one. Psychologists are not agreed as to the best way to conceive of the relation of feelings to sensations. Feeling-tone is in some ways dependent upon sensations; and at the same time, in comparison with other sensation attributes, it is relatively independent. Physiologists are still farther from agreement with regard to the nervous processes involved.

But the *deeply organic seat* of feelings is unquestioned. However the concept of feeling itself may differ, all are looking for corresponding bodily processes by means of which to classify these affective states. Clearly, to say that feeling is of such a nature that one need never hope to be able to predict it from psycho-physical conditions, is no more justified than to say that we can never predict exactly the intensity nor the vividness of any stimulation. Feeling-tone is here simply on a par with other attributes ascribed to sensation.

According to Münsterberg's Action Theory the intensity of the sensation depends upon the strength of the incoming current. Its quality depends upon the position or location of this current in its particular neurone. The vividness depends upon the "openness" or "closedness" of the neurone conditioning the *outgoing current.* And finally to the feeling-tone shall correspond the local difference of this discharge in outgoing currents. For instance, the pleasant feelings have, related to them, central outgoing paths which lead to approach, and thus to the continuation of the stimulus, and the unpleasant feelings have related to them in turn central neurones which lead

to withdrawal or escape, and thus to the breaking-up of the stimulus.

Our empirical data gathered from the experiments above reported demand not so much a modification as an elaboration of this theory. The *tridimensionality* of the feeling-tone itself must be physiologically described. We must conceive the feeling-tone itself as possessed of its own vividness, intensity, and quality.

It seems clear indeed that any explanation of the affective or feeling-character of experience must be sought somewhere in the outgoing currents from the motor region. This alone will serve to account for the inevitable volitional or "intent" aspect which invariably accompanies feeling, and I think may serve to account also for the organic or necessarily coördinating or functioning aspect required by some writers who so stoutly object to "barren atomistic or structural" psychological explanations.

The Action Theory might then be specialized in the following way:

The *intensity* of the feeling will depend upon the force or amount of the outgoing currents from the motor cells. This would enable one to explain that state of mind when a sensation only is experienced from a stimulus which ordinarily has a characteristic feeling-tone, but which feeling-tone in the special instance is lacking. Many cases have been cited above where one feeling seemed to efface another. The nerve-energy called for in arousing the unpleasant feeling-tone for the sandpaper inhibited the process of the discharge from the cells conditioning any response to the ordinarily pleasing red color. Others again can reënforce or at least not seriously interfere with each other. All cases already cited where two feeling-tones were detected as existing simultaneously are examples in point. It is quite clear of course that the intensity of feeling is not at all commensurate with the intensity of sensation. Commotion is not the only condition for emotion. Yet where there is no tendency to *do* anything, as is so noticeable in the reported introspections above, there is no feeling. A mere shock, even though intense as a sensation, simply benumbs one. In thus describing any feeling for a particular stimulation, one should include, besides the original results of the chosen peripheral excitation, all the reënforcing factors that accumulate by reason of the sensory counterparts to this originally called-for movement. When one is, for example, feeling sandpaper, the feeling for the soft red, when it exists at all, is less intense. Subjects say, "It ought to be more pleasant than it is. The trouble is in me, not in the color." The suggested movement which conditions the intensity is lessened in amount, or partially

inhibited. One could scarcely say, so far as the sensation is concerned, that it has lost some of its brightness, or that it is not strong enough to arouse its customary feeling-tone. This is distinctly reported as not the case. It is of course almost always recognized as the same shade of color. The recorded examples, showing that intensity of feeling is itself one dimension of a feeling-tone in no way necessarily related to the intensity of the sensation, are numerous.

The *vividness* of the feeling-tone is likewise a relatively independent phenomenon, and it, too, is not commensurate with the vividness of the sensation as such, and hence demands a different explanation. It can then be dependent upon the *actual stage in the process of completing the movements suggested* by the color or tone or form in question. All feelings dealt with in this investigation one can describe by relating them to the actual stage in the process of completing the *coördinated adjustments.* Without some progress in such a process no feeling would cross the threshold of awareness. In Part III above are recorded many illustrations, where *degrees of vividness* for feelings are noted by the subjects. When they were attempting to report the actual time when a feeling became definite enough to be called such at all, there was much difficulty in knowing just when to give the signal. Feelings develop much more slowly than do perceptions. Subjects often give the signal too soon, at once correcting themselves by saying that it was too vague at that moment. It grows in definiteness, and has degrees of vividness. A movement in the first stages of the process, before the feeling-tone has sufficiently developed, is a state of vague feeling. Again, many states of so-called indifferent feeling meant, according to the subjects, not lack of feeling, but rather vagueness, lack of vividness. Three or more stimulations from different sources resulted in confusion where no feelings were vivid. When the color again, for example, is pronounced "dead" so far as feeling is concerned, other feelings and other movements are too prominent. The sensations are in such cases unchanged. The intensity and vividness of the feeling-tone for the color are at a minimum.

And thirdly the *quality* of the feeling-tone must be dependent upon, and must be described in terms of the particular kind of coördinated movements suggested or actually carried out. Thus the characters of the feeling-tones for the yellow color above described, for the upright ovals, for the very high tones, for the *Laocoön* group, and for the *Horse Fair*, are in some respects alike. They have the same general *Gefühlsgrundlage.* The qualities of the feelings for soft deep red, for tactual plush, for low tones, and for the *Angelus*, and, for most sub-

jects, for Venus, would represent another class having the same *Gefühlsgrundlage.* This admits of all the uniqueness specific feelings may have, and at the same time permits of a general classification and description. Some subjects, D and F, for example, may have a feeling whose quality is disgust at some color-form combination. The accompanying sensations may be localized, as they frequently are, in the arms, with impulses to "ward off" the displeasing influence. Subject B often for the same feeling finds sensations of contraction in the throat most prominent, and subject A a stiffening of the features and incipient scowl. The most prominent localization depends upon the habits of the person and the habitual kind of reaction he has acquired and developed during his lifetime. The localization of muscular activity may differ, but *the kind of coördination does not, so far as our introspection shows.* The *regularity*, the *rate*, the *smooth light ease*, or the *heavy*, *ponderous*, *deep-seated character* of the suggested responses indicate some of the terms which would serve as aids in classifying kinds of processes which are physiological conditions for feelings of definite character. Again, feelings of pleasant repose, of depression, or of sudden collapse are still changes also in innervation tonus. These are adaptations for situations just as are the more positive or aggressive kinds illustrated above. Feelings where quick collapse occurs differ in *quality* from feelings of calm repose. All can be conceived as kinds of adaptations or responses, and clearly correspond to the characters of feeling-tones rather than to any other dimension of feelings or sensations.

Certainly the central preparedness for discharge largely determines the feelings. The external excitations are merely the clues. The internal apparatus is set vibrating in a constant manner if no other external or central stimulus is present to demand other adjustments or to intensify the same kind. When such synergetic or antagonistic stimuli are also present the mutual influences of feelings do seem to be, indeed, of great significance.

THE ÆSTHETICS OF REPEATED SPACE FORMS

BY ELEANOR HARRIS ROWLAND

PART I

THE object of this paper is to discover some of the sources of our pleasure in repeated space forms, and the laws which govern this repetition. The repetition of an object, and its regular recurrence subject to certain possible variations, is one of the basal principles of art, and of architecture in particular.

It is necessary at the outset to define our use of the word *repetition* more exactly, for there are obviously different meanings of the word, which may lead to confusion.

1. The term repetition may be applied to the existence of any two objects similar to each other, whether they are near together or widely sundered. Our pleasure in such a repetition would be merely that of re-seeing and recognizing the two as counterparts of each other. This kind of repetition I call *conceptual*, for it requires only that the memory-picture of the object be held in mind and the two recognized as similar when met again. This is not the kind of repetition which I have in mind, and I shall never use the word in this sense during the discussion.

2. In any one work of art there may be some feature repeated, some motif which is taken up and carried out in different ways throughout the whole, and these features we recognize as having an orderly relation to one another in the unity. This might be termed repetition of *content*, and be applied to the recurrence of some type of decoration over a window or a peculiar arch taken up in various ways throughout a cathedral. I do not use the word in this sense, but limit it still further.

3. By *repetition* is meant during this discussion the regular recurrence of an object, and an equally regular recurrence of intervals. The repeated object must come at uniform intervals, and this restricts us to the consideration of that repetition alone which consists of recurrence at regular intervals of some object more or less

beautiful in itself, and the description of the nature of our æsthetic feeling in experiencing such a series.

Although this discussion is divided into the two divisions of *experiments* and *analysis* of architectural examples, and the experiments are described first, the investigation was not carried out separately in this order. The two went along together, the art-analysis suggesting experiments, and the experiments in turn throwing light on the analysis. The two parts of the discussion are kept separate merely for the convenience of the reader, and in the experimental discussion all allusions to the art-illustrations are excluded in order to avoid confusion. In reality the two went hand in hand, but the connection between the experiments and art-analysis will be reserved for the latter half of the paper.

The experiments were begun in the following manner: In a velvet screen about a foot high was cut a window 460 mm. by 35 mm. in size. Behind the window was a metre measure and a rod from which hung small strips of cardboard 10 mm. wide. First two, three, and four strips were hung behind the window, and the subjects were required to arrange them at the intervals where they preferred to see them repeated. The results were uniform in certain particulars and very suggestive. In their arrangements of two, three and four strips, the subjects were guided by considerations of symmetry or proportion. They insisted that although they *knew* that the strips were repeated, they did not feel the repetition, but the strips seemed like parts of some larger unity to be arranged with reference to the unity of the whole. With the addition of the fifth strip came a difference in their apperception. Instead of the strips seeming parts of a whole including figure they seemed like repeated units.

FAVORITE ARRANGEMENTS

	2 strips	*3 strips*	*4 strips*	*5 strips*
J.	30 mms.	4 mms.	mid. sp. = 25 ends = 10	any symmetrical arrangement better than equality
S.	170	12	mid. sp. = 15 ends = 12	mid. sp. = 40 ends = 30
U.	40	20	30	35
R.	30	130	mid. sp. = 30 ends = 10	10
L.	23	40	70	70
W.	40	10	30	30
V.	20	10	mid. sp. = 100 ends = 60	15

It will be seen, from the table, that with two exceptions they preferred five strips equally distant from one another, while with four strips, four subjects had preferred a symmetrical arrangement. These gave as their reason that with five strips the latter appeared more definitely to be repetitions of one another, while the four strips seemed more like parts of a whole which required symmetry in its arrangement. Moreover the two subjects who preferred five strips in symmetrical arrangement instead of at equal distances affirmed that a distinct feeling of repetition came with five strips that had not been felt before, only they did not enjoy this feeling of repetition as well as one of symmetry. After having seen the five strips, some subjects could feel the repetition with four strips, but none with three. The question naturally arose, what is this *feeling* of repetition which makes one say that four or five repeated objects deserve the name, while three or less are regarded in a different light? The analogy between the apperception of this visual repetition and auditory rhythm seemed so strong as to deserve attention.

In auditory rhythm it is necessary that there be recurrence of more than two elements; they must come at a certain rate and within a certain temporal space to seem connected with each other, and they may be subjectively grouped in different ways. The apperception of both kinds of repetition had so many analogies as to suggest that some of the factors in both experiences were identical.

To focus the problem I took a definite thesis in regard to it. Our apperception of repeated space forms is due to the rhythm of our own motor adjustments which are excited in face of repetition, harmoniously if they accord with certain rhythmic laws in us, inharmoniously if they do not. It was then necessary to find what facts would support such a thesis, to see if in reality such facts could be marshalled, and if the explanation and support they offered was conclusive enough to make it needless to look farther.

It would seem, if our pleasure in repetition depended on temporal motor responses in us, that if the amount of time normally taken to traverse a repeated series were shortened, or if the eyes were fixed and not allowed to move over the field at all, our enjoyment would cease altogether, or at least be seriously diminished. If we found it impossible to enjoy the series except when seen for a certain time, long enough for the eyes to go over it in the rhythm peculiar to each subject, we should then conclude that our enjoyment did depend, to some extent, on such temporal rhythm.

I experimented on this question with nine subjects, and the re-

sults brought out different ways of apperceiving repetition, which divided the observers into two rather well-marked types.

The apparatus was of the simplest, consisting of white silk strings hung on a wire against a black background across one side of the room. The strings were attached to the wire by little hooks, which enabled one to change their position easily, while a cloth hid the weights on the ends of the strings, so that nothing but the vertical white lines were visible.

Fifty strings (50 mm. apart) were hung before the subjects, and they were asked to survey the field and give a signal as soon as the experience became pleasant. Then having found the average length of time for each subject to enjoy these simple repetitions, a shorter period was given when they were to shut their eyes at a given signal, and see if in that shortened time they were still able to enjoy the series. Next they fixated the eyes and kept the whole body rigid, to see if pleasure was still possible when all outward motor response was checked, so far as possible.

The results of this experiment were very suggestive. Of nine subjects, all felt pleasure when allowed to move the eyes over the series at random; with eyes fixed, five felt their pleasure much altered in its quality as well as lessened, while with one it was altogether destroyed. With four, however, although there was considerable alteration in the quality of the pleasure, its amount was increased rather than lessened.

B. (1) Average time necessary to enjoy the series: 4.7 seconds.
(2) Three-second exposure. No pleasure, needs more time during the movement.
(3) Eyes fixed: 4 secs. = Av. time necessary to enjoy it. Lines bunch toward centre and fade away at sides, giving a kind of unity, but he feels constraint.

R. (1) Av. time: 4.3. Sees them in pairs.
(2) Two-sec. exposure. Very faint pleasure; feels that only a part is perceived.
(3) Eyes fixed: 4.3. One pair fixated, the others fade away, making a kind of figure. Pleasure faint and constrained.

L. (1) Av. time: 2.1.
(2) 1-sec. exposure. Pleasure faint and incomplete. He feels the pleasure comes from memory of the previous experience.
(3) Eyes fixed: 2.2. Great effort to find any pleasure. It consists mainly in seeing a few strings, and feeling there are others, even though they are not distinguished.

V. (1) Av. time: 2.2. Sees them in pairs.
(2) 1.5-sec. exposure. Enjoys the experience in memory after the eyes are shut again.
(3) Eyes fixed: 1.9. Still sees them in pairs, but cannot see enough of them, hence they are less pleasant.

W. (1) Av. time: 4.3.
   (2) 2-sec. exposure. Not enough time to feel any relation between the strings, most of the pleasure supplied by the memory.
   (3) Eyes fixed: 5.3. Pleasure is very faint, and consists in having the strings appear to converge to a central point, and fade at the sides.

J. (1) Av. time: 2.3. Sees them in pairs.
   (2) 1.5 exposure. Less pleasant.
   (3) Eyes fixed: 2.7. Series seems more like a unity and he enjoys it more, since no time is spent in exploring the field, but it is one unified experience.

U. (1) Av. time: 28. Only enjoys it by ignoring all except those in the centre — does not want so many.
   (3) Eyes fixed: 18 secs. Enjoys it when eye lights on one string, so that the others can fade away equally at the sides, in one figure.

S. (1) Av. time: 5.
   (2) 3-sec. exposure. Less pleasant.
   (3) Eyes fixed: 8.8. Pleasure consists in converging of lines toward central point. It appears like one figure and is more intense than (1).

H. (1) Av. time: 9. Sees them in pairs.
   (2) 1 sec. Just as pleasant as before.
   (3) Eyes fixed: 4.6. Pleasure in unity of whole series with centre of fixation emphasized. Only felt pleasure anyhow when the eyes had stopped moving, so now it comes all the sooner.

From these introspections it is obvious that there are two distinct ways of apperceiving repetition: One in which the rhythmic element is pronounced, so that when the time necessary for such a rhythm is shortened, or by fixating the eyes the motor response is hindered, the pleasure in the repetition is either altered or destroyed altogether. The other type takes a repeated series in the sense of a unified presentation and wants it all at once in a symmetrical whole. The rhythmic factor is present in both, as is shown by the fact that the quality of the pleasure was changed in every case when the time of exposure was shortened. But in the latter type of subject the pleasure felt in the presentation of the whole at once, and the feeling of symmetry around a middle point, are more intense than a rhythmic apperception. These two kinds of apperception remain fairly constant throughout the experiments, and for convenience' sake we shall call them spatial and temporal types. With the former, the value of the experience consists especially in having a central fixation-point from which the repeated elements fade away equally on the sides, making a symmetrical whole. With the temporal type, the pleasure is felt by means of the rhythmical passage from one element of the series to the other. In passing from point to point the rest of the field still remains in indirect vision, so to the distinctly temporal a distinctly spatial factor is also pre-

sent. For this reason the temporal type of apperception is the richer of the two, and a description of it comes more nearly to the essence of repetition as such.

Up to this time, the repeated element had always been a single string. This was varied and the strings hung in pairs (50 mm. wide, 100 mm. between pairs). When the strings had hung at equal distances from each other, six out of the nine subjects had seen them in pairs while enjoying them, and had found such grouping more or less essential to enjoyment. In seven cases the pleasure was increased by this grouping. They expressed their preference in various ways: "Easier to keep track of where we are going." "Can go quicker over field, for repetitions are more well-marked." "Single line is too thin to rest on, this gives broader space for repose." All these introspections instanced the necessity of the rhythm being marked and made plain, so that there should be no confusion of point with point. The two who disliked this grouping were of the spatial type, who found no pleasure in traversing the field, hence too little content in it, in this arrangement, at any one time. Grouping of some kind would seem to faciliate the apperception to a certain class of subjects, while with others the amount and quality of the content of the field is of more consequence.

Since accents are such an important factor of auditory rhythm, the next experiment was to see if the apperception of a series of repeated elements would be facilitated by accenting every other one.

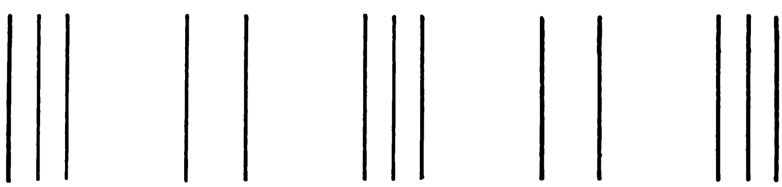


Fig. 1

Another string was hung in every other pair thus making it more striking, but here came a difference between the feeling of accent in auditory and visual rhythms. The subjects declared the pairs in which a third line was hung were not intensified alone, as when a greater stress is put on a tone in auditory rhythm, but the pairs were changed qualitatively. The group of three became the repeated element, while the pair was only an alternating figure different from the principal unit. This unanimous testimony brought up

a variety of questions. 1. Is any purely *intensive* accent, without involving qualitative changes, possible in visual repetition? 2. What factor makes us choose one object rather than another as the repeated element? 3. What is the value of the alternating figure in such a series? 4. What is the value of the empty space between repeated figures, and does it have as distinct a value as an alternating figure? 5. Are all the recurring objects and spaces felt as separate repetitions; if so, how many can be carried on at once?

These questions were put to the subjects in regard to the series just described with considerable uniformity of answer.

(1) No such thing as purely stress accent seemed possible. The word, signifying greater intensity without change of quality, did not apply. If one attempted to *intensify* the repeated object in any way, either hanging another string, thickening the strings, or any similar device, it ceased to be the old unit but became a new one, whose repetition was followed for its own sake, while the weaker one retired into the background, and was not *felt* as the element repeated in the series.

(2) Any regular change of the element which made it more interesting or caught the attention, fixed it as the chosen unit of the series, whose repetition was followed.

(3) Concerning the exact *value* of the alternating figure in the series, there was great difficulty in introspection. They all "*knew* the alternate figure was just as truly repeated as the principal one, but could not feel it so." The three-group formed the unit of the repeated series, and although the pair was clearly part of the experience and distinctly perceived, for some reason it was not felt as repeated in the same way as the other. It was merely an alternate, a filling, which was essential to the other, but which had no significance in itself as a repeated thing. Two subjects were able (if they tried) to carry both repetitions along together, *i. e.*, not only feel the three-groups as coming at regular distances from each other, but the pairs as forming another interlacing series. This kind of apperception was very fatiguing, however, and they could not enjoy it. For any pleasure to be derived, the pairs must retire into the background, and attention be fastened on the three-group.

(4) If the alternating figure was to be so subordinate, was there any difference between its significance, and that of an empty space? This was everywhere answered in the same way. The alternating, or minor figure, had a very distinct value, and any irregularity in it was even more irritating than in the principal unit itself. When

the space was empty they thought nothing of it, the equality of the interspacing was taken for granted; while if they felt an irregularity in it, it destroyed their pleasure in the whole series. But there was no feeling for the empty space until its regularity had been violated, while there was a distinctly pleasant factor in the minor figure, even though different in quality from the principal element.

In the foregoing experiment the differences between the spatial and temporal type of observer were still strongly marked. The former type invariably grouped the elements (usually with the three-group in the centre and a pair on each side) and they took their pleasure in the symmetry of each figure so made, moving from the centre of one to the centre of the next adjacent. In this method of apperception there was no empty space between repetitions, for the whole group of three figures was taken as the repeated unit. The empty or rest-phase was gotten in moving from the centre of one to the next, in which passage the limiting pair was ignored. One spatial subject, finding the proportions of this artificial grouping poor, got no enjoyment at all.

With the temporal type, the experience was quite different. They moved across the field with the three-group as their stopping-points. These principal elements were what they looked for, and their pleasure seemed to consist in expecting and meeting it. What part the *pair* played, they had difficulty in analyzing. Some said that while the three-groups occupied most of their attention, they gave a lesser degree to the pair, so that the rhythm of the passage across was marked by heavier and lighter beats. Another found the figure in the alternate space only an obstacle, and felt he was hindered in the passage from unit to unit, the only compensation being, that the "hindrance" came at regular distances. Others felt that two repetitions were actually being carried on at once. By this they meant that the two sets of elements were kept distinct, although objectively combined, but the repetition of the pair was subordinate in interest to that of the three-groups.

I tested the same thing (accents or major elements, and the value of the alternating minor elements) by simply doubling every other string of the series 50 mm. apart. In every case the effect was found poor. It was "confusing," "too much work." They all felt adjusted to the repetition of the double string and then encountered the single one, which hindered them, and by trying to keep both elements going at once they were fatigued. Most of them had a distinct feeling that they wanted to swing from one element to

the next, and were baffled by the alternate. In this arrangement, even members of the spatial type who had not been able to get any rhythmic feeling before, felt the movement in the series as if they were going across, although they went (in two cases) in groups instead of single elements. They all, however, felt the single string as an obstacle which hindered their enjoyment, whether the double string or a group was taken as element. This suggested the question: What makes the difference between the minor figure being an enrichment to the experience and being a hindrance? They insisted some rest-period was necessary; some really empty space between the repeated units, and when in place of rest they had more to do, it spoilt the pleasure.

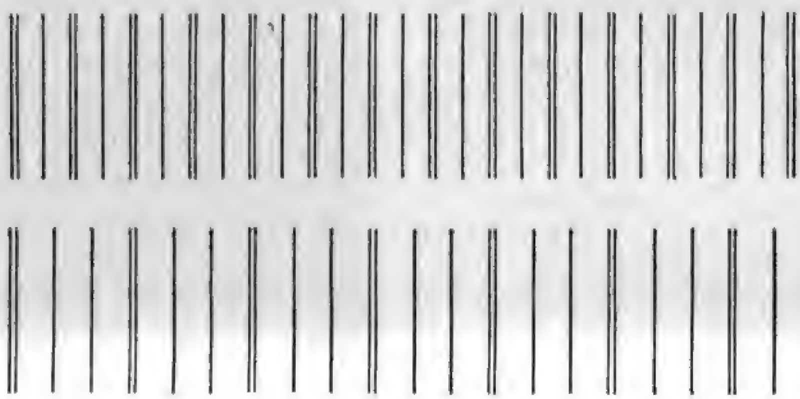


Fig. 2

Next, two strings were hung at equal distances between the double strings, and the latter put 100 mm. instead of 50 mm. apart. This was liked better in every case, and the reasons given were much alike. The double string was still the repeated unit, while the two strings between did not *feel* repeated. In spite of the obvious inconsistency of the statement that they did not *feel* the alternates to be repeated even when they knew that they were so, just as much as the double string, several subjects made the same remark. V. felt the series as a rhythm, where the double strings were all he was interested in, although he knew he should notice if the others were changed. B. could not detect that they were of any importance, except as he imagined them absent. B. could feel either the double strings or the two strings between them as the major unit, only, whichever one he took, the other retired into obscurity. He felt the minor units

in a different way as being repeated together with the majors, but very weakly, and not at all unless he previously considered that he ought to do so. L. said his attention was fastened on the double strings, but it was the "effort or ease with which he passed over the alternates which formed the pleasure." Another temporal subject felt the rhythm as the others did, with more emphasis on the double strings. But the major units seemed the *rest-phase* in his rhythm, *i. e.*, he paused here in observation of the series, although his attention was most active; *vice versa*, the minor units required little attention, but were the active or moving parts of the rhythm. Since they all considered the strings in the alternate space merely as steps or lesser beats on the way to the major element which they sought, and as obstacles rather than otherwise, why did they prefer the space with two strings rather than one? This suggested that some factor in the alternate spaces was important other than simply the amount of resistance to overcome in getting past the two lines. In answer to this they could only say that the two strings in the alternate space formed a pleasanter unity by itself, although, as they went across, they did not think of it in terms of unity.

What, then, is the real rest-phase of the rhythm of alternating repeated objects?

In the beginning of the discussion, when the analogy between visual and auditory rhythm was felt so strongly in a certain type of subject, they had expressed themselves as if the object which they called the unit of the repeated series were the active stimulated part of the experience, while the alternate space was the rest-phase, valuable only as a period of repose or blankness before the object was again encountered. But in this case, although they felt they were putting no attention or emphasis on this space, in reality they were keenly alive to what was hung in it, even preferring more "hindrance" in the way of strings than less, which suggested that the alternate space was of more value than they were conscious of. (Some of this increase in pleasure was of course due to the increased actual distance between the double strings, but some also to the extra string.) The introspection on this question as to which part of the rhythm was actually the rest-phase (if there were any such) was difficult for them all. They felt they spent more *time* on the major element; that was what they looked for and found pleasure in meeting again.

One said, "The unit is what I look for; as soon as I have it the pleasure ends and I want to move on again. The pleasure does not

consist in resting on it after it is found, but in knowing I am going to meet it again, and in doing so." As to the alternate spaces, he could only say he was not consciously interested in them, he took them for granted, but knew he should feel it, if they were changed. His feeling for them was wholly negative. The other temporal subjects agreed essentially with this. The alternate figures had to be passed, but passing them was only of importance as it helped or hindered the perceiving of the major elements. All agreed that any change was noticed and felt irritating at once, although they could not understand how it should, since so little attention was paid them normally. One subject felt the alternating strings only as obstacles between the doubles, and demanded an actual, empty rest-period between any repeated units. When asked if it were really the rest-period *between* elements or *on* them, he said he felt there was a complete discharge of attention on the major units, and an attempted one on the minor or alternate units, and the attempted ones became confused.

These introspections would point to the fact that alternate minor spaces while affording rest for the attention were periods of activity of some other kind. The fact that no one could say *what* kind, and yet insisted on the feeling of its being important and distinctive, and moreover repudiated the idea of change in a minor space even more than in a major — this seemed to show that there was a value in the alternate spaces quite aside from attention, but fully as distinct in its own way.

As might be expected, those of my subjects to whom rhythm was not a conscious factor of the experience of repetition could not understand exactly what was meant by the distinction between *rest-phase* and *emphasis* of rhythm. In all the preceding cases where the temporal type gave the introspection I have described, the spatial subjects grouped the single lines, in Fig. 2, about the heavier pair as centre, and moved from the centre of one such group to the next. The experience then consisted of a succession of adjacent symmetrical groups, connected by movement from centre to centre. When asked if there was no pleasure in finding equal distances between their centres, *i. e.*, any temporal element whatever, they all denied feeling any. They could not detect that they *felt* the distances between their centres as equal, although they *knew* them to be. They spent so much attention on the group that all feeling of the distance between its centre and the last was lost before going on to the next.

These two marked types of apperception of an alternating series seem varieties of *emphasis*, rather than of actual experience. It was evident that those in the spatial type must have some recollection of the amount of distance passed over between the various groups to feel the whole series as connected in any way; while those of the temporal type could not be wholly absorbed with the separate lines of the series as they traversed it, but were distinctly conscious of the space relations of those in the side of the field that they had just passed or were coming to.

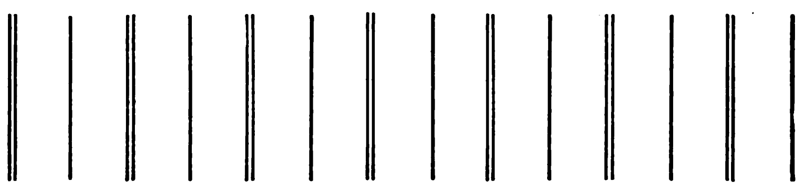


Fig. 3

Next, I tried to see what were the different factors which made up the value of the minor spaces. By varying both their size and filling, and doing the same to the major element, I could judge the relative value of these changes on the two, and their effect on the whole series.

The test was made in the following manner. The series as it stood consisted of a double line alternating with a single one.

With every temporal subject the double line was conceived to be the repeated thing, and the space between considered as an alternate, with a repeated line of its own, to be sure, but not felt in the same way as the other. With the spatial type, the single line was merely the limiting edge of the symmetrical figure, with a double line in the centre. One subject varied back and forth in his method of apperception, and considered the richness and variety of these different apperceptions as one of the chief sources of the pleasure therein.

Variation of alternating spaces: The minor spaces were varied by hanging two strings in one, and one in the other, and subject asked how such a change affected his feeling for them. The change was marked.

The spaces which had before been minor were so no longer. The alternate space in which two strings were hung with the boundary-line of the two double strings became the new element, and the alternate in which only *one* string was hung continued to be the

Fig. 4

alternate in the new series. The whole series shifted itself, and settled into a new equilibrium. Some of the subjects were able to feel all the former minor spaces still as such, but only by a definite effort, and not while taking any pleasure in it. The change in the alternates spoilt the whole scheme of the repetition as it already stood, and made a regrouping necessary. I next tried varying alternates by removing a string at intervals.

Fig. 5

Since the strings were not removed in any regular fashion, and because the subject could not find any possible consecutive way to group them with the double strings, this variation was partially overlooked, and although confusing the series somewhat, repetition of the double strings could still be felt. Thus a mere *gap* where the scheme remained the same was not so disturbing as an extra feature inserted, or one noticeably changed. Something could be supplied by the subject, but not altered so easily. In these cases, however, the change was only tolerated because it was ignored. They felt it as a mistake and so overlooked it, but, accepted as a component part of the series, it was impossible to feel it as a repetition or get pleasure from it.

Fig. 6

The next variation was in the position of alternate figures. With a three-group as the major element of the repetition and a pair of strings in the alternate space, the size of the two minor spaces was altered, thus making the distance between the three-group and adjacent pair shorter than between that and the next three-group. This immediately threw out the feeling for the old series and made a new one. The new series thus formed varied with the different subjects, although no particular difference was noticed between spatial and temporal types. They all disliked the new arrangement, in whichever of a variety of ways it was apperceived. (It will be noted the actual distance between the three-groups was not varied, but the size of the spaces each side of the minor figure, *i. e.*, the minor figure was shifted from its central position.) One typical spatial subject took it in either of three ways: (1) He grouped the three-group and pair nearest together, into the repeated element of the new series; (2) he ignored the pair and regarded it as a repetition of three-groups; or (3) ignored the difference in the division of the alternates, and regarded them as alike. The artificiality of the latter methods of taking the series is evident. What pleasure survived after such a strain was very slight, and was moreover not of the series as given, but as imagined differently, which was not a valid judgment. Most of the subjects grouped both figures into one, and, finding the unity thus made ugly and unsymmetrical, derived no pleasure from it. One tried to keep both elements in separate series and have them go along together, equally distant from those of their own kind, although not from each other. This was, however, very fatiguing and unsatisfactory. Those who grouped the different figures said they did so because they could not help it, not because they liked it, and said it was impossible to regard the alternate figure as such, if varied from its central position. If they were all varied together, they were grouped, with the major unit, into a new one. If varied irregularly the series was spoiled — no rhythm whatever remained. It became a heap.

Next, I tried varying the size of the alternate spaces, keeping the filling in its central position.

Fig. 7

Here also it was universally regrouped. They found it more difficult to feel the rhythm of the three-groups as separate elements than when the minor spaces had remained uniform in size, but different in the position of the filling. The alternate space, then, which had at first seemed the unimportant part of the rhythm and for which no subject could assign any conscious value whatever, was evidently a potent factor of the experience, and when varied either in size or filling (especially the former) it not only changed the feeling-tone, but shifted the entire scheme of the rhythm, or broke it up altogether.

Variation of major units: Was variation more allowable in the major than in the minor unit of a series? This was tested first in the same manner as for accents. In a series of which a double string was the major element, a third string was hung with every such double, thus changing the unit in both size and content.

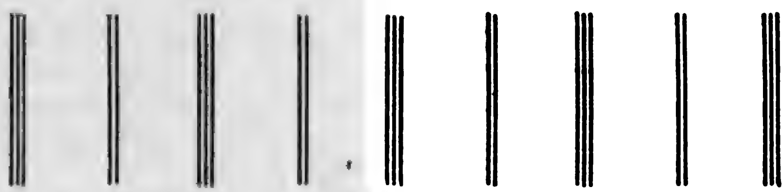


Fig. 8

The series immediately readjusted itself with the three-group as element for most of the subjects, although one was still able to feel them all as one unit, varied by the added string. Varying only the *size* of the major unit gave the same result.

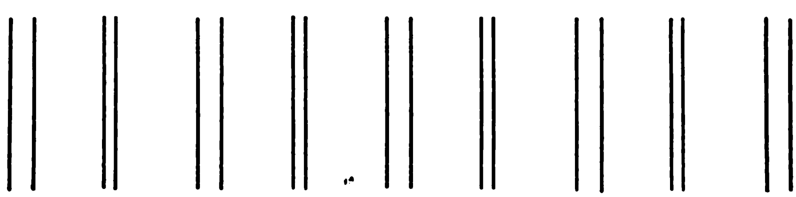


Fig. 9

The pairs, instead of remaining the same size, were made alternately larger and smaller, and a new repetition was made, *i. e.*, with the larger pair as major element and the smaller one as minor. They all agreed, however, that less change was made in these cases than when the *minor* spaces had been changed in size. In the latter case either a regrouping was made, changing the whole character

of the series, or it was spoiled altogether. With change of *major* units alone, however, although a new element was made, it was still possible to take it in the old way without much difficulty or change in feeling-tone.

It was then necessary to see how change of content would affect the major unit, the size remaining constant. A group of two sets of double strings 10 ccm. apart was taken as the repeated element, and these groups placed at 10 ccm. from each other.

Fig. 10

Within one element was hung one string, and within the next two, thus varying the content while the size remained constant. In every case the answer was the same. It was not so pleasant as when the filling was the same, but the group still remained the unit of the repetition, and the series essentially the same.

Several variations were made in this element. Instead of hanging strings regularly (1 in one, and 2 in the next) they were hung irregularly, *i. e.*, an extra one here and there at intervals in no special order. As long as the boundary-lines of each group remained at the same distance from each other, and from the next group, thus keeping the unit at uniform size, although the pleasure-tone varied, the balance of the series was not changed. No regrouping or shifting of the equilibrium resulted.

It would seem from the preceding experiments that in any series variation of the *major unit* was tolerated more than of the *alternate;* while in either case variation in *content* had less influence than variation in *size.*

Symmetry: In the previous experiment, three subjects had insisted on symmetry as a necessary attribute both of the unit and its alternate. U. (spatial type) described his experience as "a succession of symmetrical experiences or states of equilibrium; when they are not so, they must be regrouped, or pleasure is impossible." R. (temporal type) insisted especially on the necessity of the alternate figure being symmetrical as regards the major units, *i. e.*, halfway between them; and also on symmetry as regards itself. One

temporal subject said there was some pleasure in merely going from one unit to the next, even though no repose was possible on each because of its asymmetry. This suggested experiments on the importance of symmetry in repeated series. Is it necessary that the separate elements of a series be symmetrical? Must both major and minor element be symmetrical? Does this necessity vary according to the temporal or spatial type of the subject, *i. e.*, is it more necessary to the spatial type, whose pleasure depends more on repose in the unit, than to the temporal type, whose enjoyment rests mainly in the rhythm of movement from one unit to the next? Or is it a common demand? This experiment was begun in the following simple way. The strings were hung in two group-forms; one with three and the other with four.

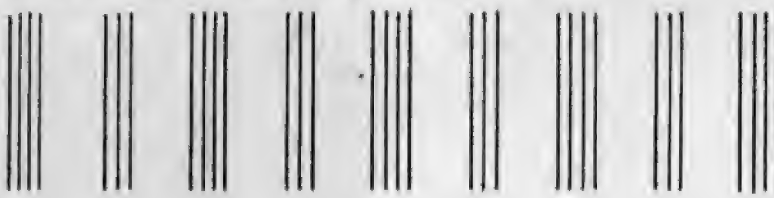


Fig. 11

This was a symmetrical grouping and uniformly pleasant. The series was then changed by removing the second string in the four-group, thereby making it unsymmetrical.

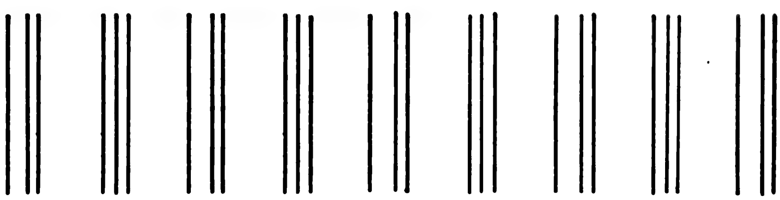


Fig. 12

This change made the repetition less pleasant in every case, but did not spoil it. Instead of the four-groups becoming more prominent they seemed less so, and the three-group on account of its "compactness" became in most cases the major element, thereby shifting the balance of the repetition, but not detracting very much from the pleasure. Next the three-group was changed by moving the middle string to the left. By this means the group which had been minor in Fig. 11, became unsymmetrical, while the four-group was regular.

This change was preferred to that in Fig. 12, although different reasons were given. One said it was because this change in arrangement made the elements more distinct, hence easier to keep apart,

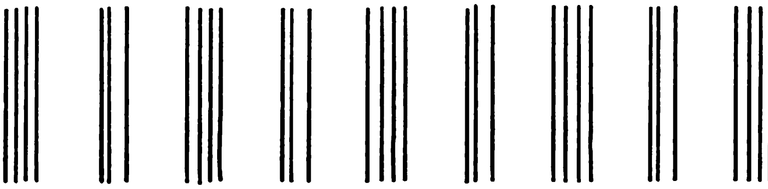


Fig. 13

while in Fig. 12 they were made more alike. Moreover, one element seemed as important as the other. He did not class them as major or minor, so he could not compare the relative values of symmetry in principal and alternate units, for in this series he did not feel the distinction. The other answers to this question were rather incoherent, but the series did not seem to suffer much change, either pleasantly or otherwise. Since lack of symmetry in *the element* was at least tolerated in the examples already given, would it be allowable so to place the units that the two adjacent to any one unit should lie unsymmetrically on either side, that is, may the elements lie unsymmetrically with regard to one another? Suppose a four-group to be repeated at regular intervals, and a three-group likewise; if the two series were combined, must they occur halfway between each other? That is, must they be symmetrically placed as regards the intervening space, or could they be put to one side?

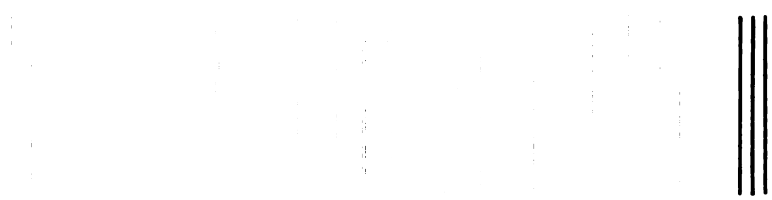


Fig. 14

The subject was asked not to group them (as in previous similar arrangements), but to keep them as separate repetitions if possible, and to see if this equal distance was necessary to keep them apart. The result was the same in all cases. The subjects could not help grouping them, and found it impossible to keep them distinct unless so much effort was put into it that no pleasure was left. They said they "*knew* each unit was as equally distant from the next

unit in its *own* series, as if it did not come at unequal distances from the units in the other, but they could not feel it so, and were obliged to group the two together." For this reason the experiments did not satisfactorily illustrate the point in question. It was necessary to have a series of elements whose unity was more strongly marked, and whose different parts would still remain one *whole* even after variations, instead of shifting into each other. It was suggested by these imperfect experiments that symmetry was *not* so important a factor in the different units of a series as the subjects had previously supposed; but that, on the other hand, the different units must be placed at equal distances from each other, if they are to be kept distinct either as two series or as one. Moreover, that *two* series could not be kept distinctly in mind as separate, *anyway*, without fatigue, the tendency being always to group them into one series with a new repeated element, composed of a combination of the other two. It was necessary, however, to test this more completely. By a simple device the former series was changed radically, so that the difficulties mentioned were overcome. The strings of both the three and four groups were twisted together at the bottom, thus binding them closely into separate unities. By remaining attached at the bottom, whatever variations might occur elsewhere in the figures, they could not lose their individuality and become merged in each other as before. They remained distinct groups without effort on the part of the subject.

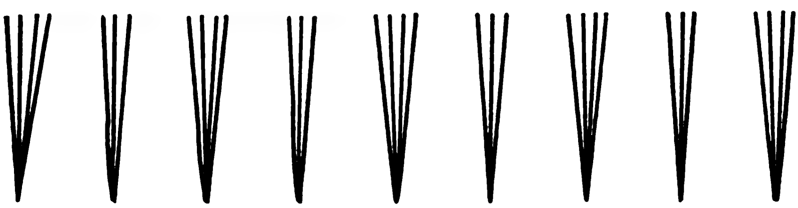


Fig. 15

With this I began as in the previous experiment. The subject was asked to look at the series of repetitions, enjoy them as much as possible, telling what was the pleasant factor in the experience, and how he apperceived the series. The subjects separated into types as before; the spatial type immediately grouping the elements into a larger unity and enjoying the groups more for their own sake than for their repetition, while the temporal type went from one to the next in the series, enjoying the *rhythm* more than the elements as such. (It may be remarked here that the subjects

were perfectly naïve as to their apperception. They did not know they were separated into types, nor were they urged to be consistent. Even the experimenter did not know of the distinctness with which these types separated themselves, and consistently held to their own method of apperception, until looking over the records afterwards.)

With the temporal type the four-group was the major group. Some expressed its prominence in terms of *time, i. e.*, they spent more time on it, and less on the three-group. One felt it as emphasized, because he moved from one four-group to the next like it, and at each step moved back and forth from left to right, to see the alternate three-groups on each side, always *resting* on the four-groups.

It is noticeable with these subjects, in whom the rhythmic element was more strongly developed, that although they admitted that the language of "temporal rhythm" did not adequately cover their experience (because the element did not disappear after perception as in auditory rhythm, but remained in the visual field), still they could not express themselves in other terms. L., the most extreme of this type, insisted that the experience of repetition would be exactly as pleasant if he saw the elements pass one by one behind a moving window, with never more than one in the field at once. In other words, their temporal relations were all he felt.

The others did not go so far as this, and agreed to the significance of the whole field, even while especially interested in passing from one to the next. B. partook of the characteristics of both types, and by combining the apperceptions of both bridged the chasm between them.

With the four spatial subjects, the apperception showed its usual divergence. Three grouped the elements, either with the three-group in the centre on account of its being more compact and graceful, or the four-group because it was heavier. One of them could group it either way, distinguishing between the prominence in an element due to *interest* and due to *beauty, i. e.*, he found the four-group more noticeable and interesting on account of its size, while the three-group was more beautiful as a unity, on account of its proportion and grace. Therefore according as one factor or the other predominated, one or the other figure was taken as the more prominent element, and placed in the centre of the group. Sometimes they separated into two series running along together, but this was not usual.

Having got these varied introspections, with yet a certain likeness running through them, the balance of the four-group and of the three-group were varied in turns, to see how the change in symmetry of elements would affect the series; and the relative value of symmetry for the major and minor units of a series.

First, the four-group was altered, by moving the second string further to the left, while the three-group remained symmetrical.

Fig. 16

In three cases this arrangement was preferred to the regular one previous, and each time for the same reason. The four-group was made more noticeable by being unsymmetrical, and hence more easily distinguished from the other. The two were easier kept apart, and the alternation between the two was made more clear-cut and obvious. With others the change was unpleasant for the reason that it affected them in an exactly opposite manner. The four-group *lost* its individuality, and, by separation into two unsymmetrical parts, could not be distinguished so well from the three-groups as formerly, hence the effect was spoiled.

A distinction was made between the relative *importance* and *interest* attached to the units, when symmetrical and when unsymmetrical. Every one agreed that making the four-group unsymmetrical gave it more prominence of a certain kind. With the first three subjects mentioned, this prominence was enough to accent the rhythm still more than before, and differentiate the two units more strongly. But with the other there was a feeling that while it gained prominence and was more *noticeable*, it lost coherence and interest, thence it could not be kept as the principal unit, but the attention passed over to the three-group which maintained its unity.

It would seem, then, that the mere fact of one unit in a series alternating with another, and being more noticeable, taking up a larger space, being more complicated, etc., did not insure its being the chief unit in the series. One subject voiced essentially the feeling of all, in his comment on the series: "There is a constant

struggle between the prominence which the four-group gains from size and eccentricity, and the insignificance which it deserves on account of its looseness and lack of unity; it cannot hold its own as one individual thing, and because the three-group still does, it becomes in one way more prominent, while the four-group remains so in another." Another subject felt he gave more *time* to the four-group than before, because being separated it would not bind together again without effort. At the same time the three-group gained in interest because it was easy to find and did not vary. Another subject felt that the *time* spent on a unit had nothing to do with its rhythm; it was all a matter of interest and attention. Often he looked a longer time at one unit, choosing another for the chief element in his series, because it interested him more.

All this introspection brought out two things clearly:

(1) The apperception of a series of alternating units, whether of the spatial or temporal type, is not fixed, but any variation of its unit is liable to shift the emphasis. Thus, as in the present case, when a symmetrical major unit is made unsymmetrical, it may not remain the principal unit, but becomes the minor one, because the attention shifts to the other which was before relatively unimportant.

(2) Whether either element shall be the principal one or not, does not depend wholly on its objective prominence, but on the amount of beauty or interest which it holds for the observer. Neither size, complexity, nor eccentricity can force a certain unit to be taken as the major in a series, unless it thereby presents an interest which makes the observer choose it.

The next change was to vary the three-group in a similar way, by pushing the middle string to the left, thereby making it unsymmetrical.

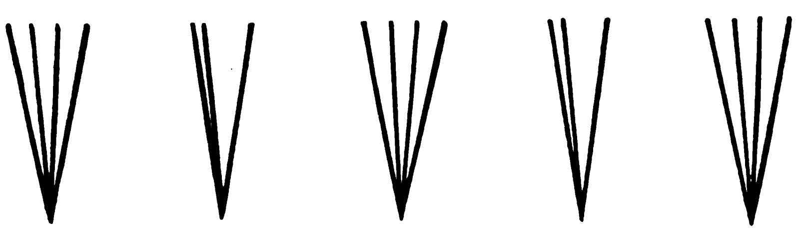


Fig. 17

The responses were as follows: Five said it did more violence to the series to have the alternate varied than the major unit; it

was more confusing. Three preferred it, giving as their reason that it made the elements more different from each other than before, hence more easily distinguished. The preponderance of evidence was, therefore, that, although any variation from symmetry in a unit was likely to be detrimental to the repetition, it was more likely to be tolerable in the major unit than in the alternate space. In either case it was demanded that the two units be distinctly different, and it depended on the individual subject, whether in this experiment the variation of one unit or the other brought out this distinction more obviously. Aside from this consideration, however, it appeared that the alternate spaces as such required equilibrium more than the principal unit. Also, variation of symmetry in the major unit, while it made it more prominent in the way of *eccentricity*, also made the symmetrical minor unit more prominent in the way of *interest*. As one subject expressed it, "Since the others vary, the attention requires something which does *not* vary, and forces prominence on the minor unit, because it remains symmetrical"; and "The minor unit is too small to merit such prominence as it gets by lack of symmetry. It is distorted, and has not enough content to bear it."

These introspections from temporal and spatial subject alike, all point to the fact of a certain *value* attached to the alternating units in a series. (1) The units must not be too much alike in *interest*, or they rival each other. (2) They must not have more prominence given them as regards the whole than they have interest to sustain. (3) There must be a congruity between the two elements so that one shall not be noticeable in one way, and one in another, thus carrying the attention in two different directions.

One more thing was suggested by this experiment: (4) The subjects who had invariably *grouped* their different elements in other series found it very difficult to do so in this, or wholly impossible. None of them did so when taking the series naturally, but moved on from one to the next just as the rhythmic type did. They felt "forced to move on," "no place to rest," while one in whom the rhythmic feeling was weakest was much fatigued by this movement, and insisted on having something stable to rest upon if he was to gain any pleasure at all.

Next the series was varied by making both units unsymmetrical; first with the balance tipping the same, and second in opposite ways.

Those who preferred this gave essentially the same reason. They

agreed that the unity of both elements was broken up by this change, and they did not stand out distinctly from each other; but all felt a certain *congruity* in having both major and minor units follow the same scheme in composition. They were not distinctly an

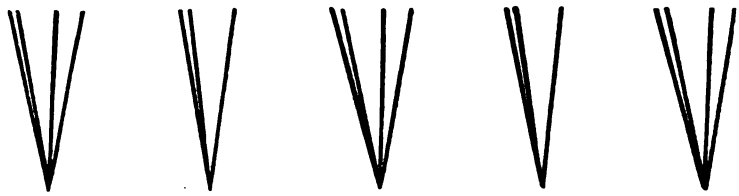


Fig. 18 A

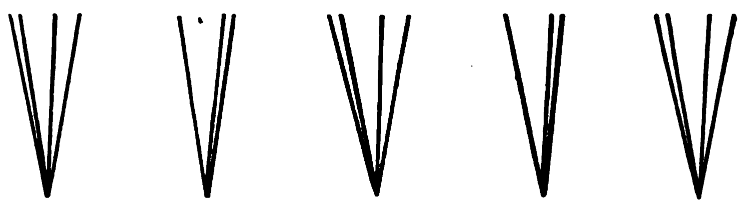


Fig. 18 B

alternating series, but harmonized better as lines. The two spatial subjects, who disliked this arrangement more than the other, gave the same reason: the unity of the elements was spoiled, they did not "hang together." Their dislike was similar in kind to that of the others, only the congruity which made up for it with the former failed to satisfy these. With the symmetry broken and the balance tipping in different ways, the feeling was not strong in either direction. They still criticised it in the same terms of congruity and distinctness, with no especial change on account of this modification.

These experiments all pointed to the fact that (1) a certain amount of congruity and equality was necessary between elements of a series (although it did not establish what were the essential features of such a harmony). (2) It is more pleasant, as a rule, to have the elements symmetrical, although symmetry was not a necessity for an agreeable series. (3) Provided the change in the symmetry of the units was not enough to shift the whole order of the series, changing the major to minor units (and *vice versa*), any varying of the symmetry of a minor unit was more disturbing to the repetition than of the major, while varying their symmetrical position, as regards the unit on either side, was absolutely destructive to the order.

The next experiment dealt with a different side of the question. Since the unit of a repeated series may be a group with repetitions inside itself, does the repetition of lines or figures *inside* the group differ from the repetition of the groups as a whole? If so, how? That is, in the enjoyment of a series of groups with repeated lines in the group, in what respect does our apperception of the repetition differ in the two cases? Or does it in reality differ at all?

To test this, the strings were arranged in the following way. 10 groups of five strings were hung 100 mm. wide and 100 mm. apart. Each unit had, then, five repetitions within it.

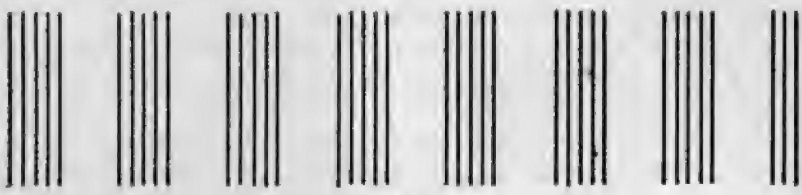


Fig. 19

The arrangement was pleasant to all the subjects, and they described the effect of the experience, falling at once into the spatial and temporal types as before (this was wholly naïve, for the same questions were asked of each, and they had no idea of being grouped in types). The introspection of both types must be taken in some detail, to fully analyze the experience.

Spatial: J. felt he took in all groups at once. Each unit seemed like a rich experience in itself, but he could not detect any rhythm in it, nor in the whole series. The pleasure consisted in getting a number of similar objects in the field at once, and enjoying the combination of them all, feeling that they stretched away in each direction. H. and U. grouped several unit-groups into a larger unity and enjoyed the cluster as a whole. They did not group them in any particular system, nor could they detect the slightest pleasure in moving from one such group to the next. One found his enjoyment solely in the contrast effects in each, while the other laid it to the space relations of each independently. The pleasure only came when each group of groups was spread before him. Those outside the immediate field meant nothing to him, and the movement between them had absolutely no conscious interest for him. S. said that enjoyment stopped altogether during motion of any kind, and the experience was pleasant only during total repose, on whatever happened to be in his field at once.

With the temporal type came a marked difference in apperception.

B. affirmed the pleasure to *consist* in going from one cluster to another, and to *begin* just at the point where he meets the next stimulation and feels it is *going to be* the same as the one previous. It is the *expectation*, rather than the *verification* recurring at intervals, which makes up the pleasure; not the actual movement, or subsequent contemplation of a group. The pleasure came in pulses; in knowing by seeing from the side of the eyes that the experience *is to be* repeated, and on reaching the edge of a new group, in the feeling that the experience is just about to begin.

R. felt that she "wriggled around" in each group of lines, and that a certain *feeling* came from "wriggling" among the lines in a particular fashion.

The pleasure consisted in having this feeling recur at regular intervals. The repetitions inside the group and of the group as a whole differed in this respect: For the separate unit-group, it apparently consisted in repeated short irregular movements, back and forth, enough to bring about a certain feeling which seemed pleasant and sufficient unto itself. Repetition of the groups as a whole meant movement across the field in one direction, for the purpose of meeting another group, and getting the required feeling from it again. The pleasure was not in the movement or in any repose (she could detect no repose at all), but in experiencing the group again, feeling that it had been so before, and would be again.

L. (the most extreme of the temporal type) agreed with R. that the lines inside the group were perceived and enjoyed *temporally*, as well as the groups as a whole. There was no experiencing the groups *at once*. He felt that he moved regularly across the field encountering five lines, one after the other, then an empty space, then five lines more. The only meaning which the group as such had for him was the five accents which came near one another in time. He could feel no unity whatever apart from this. He was even certain that his pleasure would be identical if in some mechanical way the same figure could be pushed forward, so that the same amount of time and movement would be necessary to reach it that was required to move from one figure to the next on the field. The experience was in every way analogous to auditory rhythm with him, and he was unable to express himself in other than temporal terms. Immediate perception, repose on the object, or groupings, had no significance for him.

The other two subjects were links between the extremes already described. They could feel each group, and sometimes even the whole series *at once* apparently, and yet were all the time conscious of a certain rhythm in going from one to the next. The whole experience seemed immediate at first, but on reflection a certain alternate rhythm was felt to be present, which was too rapid to take any considerable time, but yet had to be included as a factor in the experience. These introspections I believe to throw light on the nature of the whole experience of repetition. Since there are two methods of apperception so extreme, but moreover certain subjects partake of the characteristics of both, it might seem that both types represent but *one* side of the experience. Since both are enjoying the same objective series, but in their description of their feeling in face of it emphasize such different sides (leaving at the same time the other side unaccounted for), and since certain subjects share the experience of both, it might be that the sum of both methods of apperception was necessary to the fullest appreciation of the repetition in question, only in certain subjects one aspect of it was so much stronger that the other possible factors in the experience were overlooked.

It would tend to bear out this view, that when it was suggested to those of the temporal type (always excepting L.) that according to their description the other groups remaining in the field, after having performed their part in a temporal series, *ought* to have no further influence in the repetition, whereas they did in reality, they admitted the fact, but could not account for it. Moreover, those of the spatial type admitted that their enjoyment in having spaces equal, and in having repeated objects exactly like one another, had a certain character which no other experience possessed. This did not seem accounted for by any description they could give of its effect on them, although they could not detect what this other elusive factor might be.

These introspections, therefore, and the confessions on the part of both that there was a feeling of something *more* which they could not hold long enough to describe, suggested that both types were but opposite ends of a series of possible apperceptive types, and that in both cases certain essential features were emphasized at the expense of the others.

After these experiments, the next step was to find how a series of groups was apperceived when the lines in each group were arranged symmetrically about a centre, as distinguished from their

arrangement at invariable distances apart. The same number and size of groups were taken, but the arrangement of lines in each varied as stated above.

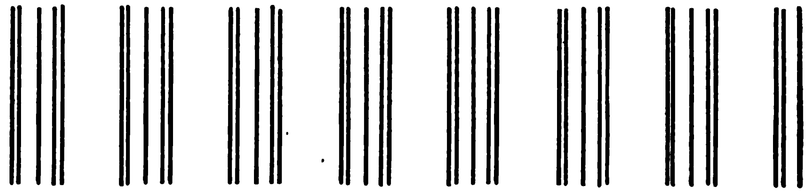


Fig. 20

Spatial subjects: J. felt a different kind of pleasure from that felt with Fig. 19. Here the enjoyment was in each unit for itself, a certain repose in its symmetry. Although he fixated on the centre of the groups, and in going across the field moved from centre to centre, there was no feeling of rhythm whatever, merely enjoyment of the unit itself. Moreover, although he had *detected* no rhythm in the previous experience, this one seemed distinctly different in having lost a feature that the other had. He felt by comparison that the other had had a temporal character, some movement in the groups, which was wholly lacking in this. This was more beautiful and restful, the other more exciting and rich.

H. and S. both enjoyed this series better on account of possibility of greater repose in the unit-groups. The pleasure was solely in each unit for itself, not in their repetition, so the group which offered most balance and equilibrium in itself was pleasantest. S. also found enjoyment in slight variations in the groups (trifling difference in distance, different light effect, etc.). It is noticeable that when repetition alone was the main feature of the series, any variation was either ignored or found unpleasant. But when the unit for itself is the object of enjoyment, variation if slight is another element of pleasure.

There is also pleasure in the mere *repeating* of symmetrical groups, although, when the attention is turned to this feature, the feeling of *symmetry* is less felt. Even when attending chiefly to the repetition of the groups, the symmetry of each is felt somewhat, which makes the whole experience better than Fig. 19, but the two attitudes seem to hinder, rather than help each other.

This introspection was suggestive, giving rise to two more questions: (1) When is variation allowable, and when not? Is it adapted only to objects when taken, as ends-in-themselves, and not when

considered as means to something else, *i. e.*, as means to make a series or border, or anything which takes attention from themselves as unities with individual meaning? (2) Is a distinctly symmetrical group as adapted to repetition, as such, as one with merely equal divisions? Does it not tend more to repose in itself, instead of to the motion necessary for the apperception of a repeated series? These questions will be considered later.

Temporal type: R. felt a difference in the movement across, in that in Fig. 20 it was from the centre of one unit to the next, while in Fig. 19 there was no regularity in the movement.

L. felt the difference between the two apperceptions very strongly. In Fig. 20 the movements seemed organized. He felt as if his attention (if not his eyes) went back and forth from edge to edge of the unit, finally settling in the centre; while in Fig. 19 the very essence of the apperception was that every line was compared with every other, meaning a great number of movements in both directions, *not* stopping in the centre. If he did rest finally in the centre, in the unit of Fig. 19, instead of seeming evenly repeated, it too became symmetrically perceived, but the usual way to get from one such unit to the next was to move *from* and *to* any point in the next adjacent, other than the central one. In either case the pleasure came in identification of the second figure with the first, and the feeling, "I have seen it before." The pleasure lay in the process of recurrence of sameness.

V. also, who had not felt much motion in Fig. 19 at first, felt it strongly now in comparison with Fig. 20. He said in the former, although he did not make actual movements across (in fact his eyes were plainly at rest), he was sure he felt "dispositions to do so" which were lacking in Fig. 20. The pleasure came in the first moment of repose after finding the new unit was the same as the old.

After we had investigated the different methods of apperceiving groups of repeated lines, and compared the effects of different groupings, and studied the feeling of one unit alternating with another, another question arose.

These questions had all referred to the alternation of *two* units; either a unit with an empty space, or with a space of different filling. How did the apperception differ when *three* repeated units alternated with each other? To test this, three spaces were taken equally wide (110 mm.) and equally distant from each other (150 mm.), but with three different designs within them. These designs

were of the same general character and importance although different, and repeated themselves regularly.

Fig. 21

The subjects were asked as before to describe their reaction on the series. Not one of all the number was able to *feel* the repetition of the three units; what pleasure they got from the series (if indeed they got any) was from other sources. The general type of answer was formulated more fully by L. He saw 1, then looked for 2 and found it different, but could have included it in the series if it were not for 3. That being still different sent 1 and 2 out of mind, so that he could not *feel* any repetition of 1 when he met it again. He felt a certain sense of repetition in that the spacing and general motifs were the same, but there was no pleasure in that. What pleasure he got was wholly intellectual, not immediate, except for a slight pleasure in their uniformity of position. In him the rhythmic feeling had always been of the strongest, but he found in this experience none whatever. It was simply impossible to keep the three units going at once. Another temporal subject tried to group 2 and 3 as one element, with one as an alternate, thus reducing it to a rhythm of twos. This process was labored, but otherwise no enjoyment was possible. The spatial subjects derived what pleasure they could, either from the units separately, with no regard to their repetition, or from some method of grouping, by which their difference could be overlooked. One expressed his pleasure solely in terms of contrast of the white strings against a black ground. Any immediate feeling for the repetition was impossible for either type. It will be noticed that the *feeling* of the repetition is quite different from the *knowing* it is there. They were all perfectly conscious that 1 was repeated again after 3, but could not *feel* it, while repeated simply after 2, they *could* feel it.

Next, the series was varied again. The size of the blocks, instead of being alike, was varied three ways, while the designs remained similar.

The interspaces were 150 mm. in every case, but 1 was 150 mm. wide; 2 = 110 mm.; 3 = 70 mm. All the spatial subjects found

Fig. 22

Fig. 22 worse than Fig. 21. The irregularity and general disorder was more pronounced. Although the rhythm had never consciously given them pleasure, and, when not violated, was never noticed, still the three-fold difference in size violates some feeling which they can only express in rhythmic terms. Some tried to group the three units into a larger group, but this being unsymmetrical displeased them. Others picked out the most satisfactorily proportioned unit and ignored the others, but any possible apperception was irritating. The temporal subjects found it equally poor. They felt the continual dissatisfaction of having their expectation, that the adjacent unit should be the same, disappointed. They all said that they could carry the feeling of repetition over *one* dissimilar unit (*i. e.*, in an alternating series of two different units), but that the third difference completely upset the scheme. When only the filling varied as in Fig. 21, it could be partially ignored, but difference in size could not be ignored, and only the equal distances apart kept them from being a heap. They could not *feel* the evenness of the empty interspaces, however. They were not consciously present in the experience at all, they merely *knew* they must be even. There was no feeling-tone whatever to the empty alternates.

Only one subject preferred Fig. 22 to Fig. 21, and the reason was obvious. 1, 2, and 3 appeared as the same unit where variation in size was apperceived as due to perspective. Thus, instead of appearing as three units repeated, they were one set which progressed by means of "pulsations" or regular intervals of perspective. This gave an added richness to the rhythm, and was very pleasant. As three separate units of different size, there was no meaning in the series whatever.

It is evident from these introspections that, although the likes and dislikes may vary, the principles on which they are based have much in common.

The points on which they agreed unanimously were the following:

(1) There is no feeling of repetition for three separate units. The series may be enjoyed by means of subjective grouping of one kind or another, but as separate elements, the feeling of repetition is broken by adding the third.

(2) There is a distinction between perceiving or *knowing* a repetition, and *feeling* it. Even though a subject is equally conscious that elements are repeated according to some scheme in two different cases, he may feel it in one case and not in the other.

(3) The empty spaces between the elements have no conscious part to play in the experience. Even when there is a figure in the alternate space, it comes very little into consciousness as part of the repetition, yet it is alterations in these alternates which make or mar the feeling of repetition. A series may not be beautiful in itself, but if the alternates are regular, it *feels* repeated. *Vice versa*, the units may be enjoyable in themselves, but they do not feel repeated unless the alternates are regular and conform to certain requirements. In the units lies the *meaning* of the repetition, in the regular alternates the possibility of its expression.

(4) The rhythmic character of repetition is not felt by a certain type of subject, when it goes smoothly. When a variation is made which would destroy any possible rhythm, its lack is felt, and its violation finds expression only in rhythmic terms.

(5) More violation is done to a series to have the *size* of units varied than the filling. (This corroborates previous experiments.)

(6) A certain amount of ignoring and regrouping can be done by the subject. The series is not taken exactly as given, but with selective attention.

(7) In a series of different elements alternating, the most prominent one is chosen for the major unit, and the others for alternates. This prominence is more influenced by *size* than any other factor, but may be due to intrinsic interest of any kind.

(8) The major and minor elements must have a certain difference from each other, both in *appearance* and *interest*, and they must be different enough for the difference to be easily perceived, but not enough to be incongruous. They must differ in interest enough, so that one is easily more prominent than the other, or may be made subservient to the other, in the apperception.

(9) Variations are pleasant in the principal unit repeated, but not in the alternating figure unless very slight indeed, or affecting only secondary parts of the figure, not the main lines.

(10) Not the time actually spent on a unit makes it more or less prominent, but the feeling of more or less "energy" expended on it.

Ends: In an alternating repetition, must the series end on a light or heavy beat? That is, must the major or minor unit be on the end?

To test this a series of strings was hung in which a group of three alternated with a single string.

Fig. 23

The subjects were asked to look at it with the three-groups on the end, and with the single string. In every case the three-group ending was emphatically declared the best. What reasons were given were much the same, although most of them could give no explanation at all. S. said the minor space on the end left him "hanging in mid-air, it needs the heavy beat to land me again." Others said it was "ragged" unless the three-group ended the series. R. said anything *interesting* would do on the end, as well as the larger-sized unit, it simply needed something of sufficient interest to stop the rhythmic process and keep one from going on.

It was impossible to describe the experience except in rhythmic terms, and those in whom this sense was not strong could give no account whatever for the difference in their feeling for *end.*

It will be remembered that some experiments were previously described relating to the difference in apperception of a group of lines equally distant from one another, and a group averaged at equal distances each side of a middle point, but unequally from each, to emphasize the bilateral symmetry. Two such series were now taken to find if there were any difference necessary in appropriate endings. Since the two types of groups differed so much in apperception, did that difference so extend to the whole series that a different space was needed at the end to finish them off?

The method of experiment was the following: Two series of repeated groups were hung (100 mm. wide and 100 mm. between) with the design of the groups varied as described. At the end of each

a strip of cardboard was hung, which the subject was asked to move so that it bounded the amount of space at the end, necessary to finish the series adequately.

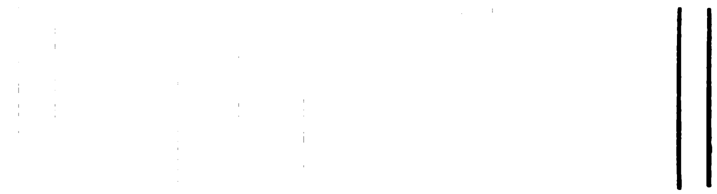


Fig. 24 I *a* *b*

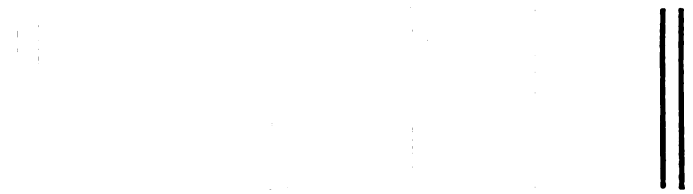


Fig. 24 II

Thus *b* is the cardboard strip, and *a* the space which was to be varied according to his taste. The same experiment was tried with each series, with the following results:

	I	II
U.	*a* = 96 mm.	*a* = 90
J.	33	50
S.	97	90
H.	109	104
R.	160	150
V.	170	135
T.	145	125
W.	80	68

In the case of every one but J. the subjects preferred a longer end space with I than with II. J. was, however, of the extreme spatial type who gave as his explanation that with II, when the central line was prominent, the end (*a*) must equal just the distance to another middle line, while with I it must harmonize with the shorter distances in the group, but not exactly equal them, for that would make it too narrow.

It would mean, then, that the apperception of the repeated group in I (if it accords with the subject's own introspection) consists in repeated fluctuations of attention over the five strings, with no repose

on any one more than another. The movement is back and forth from edge to edge, and hence needs more of an end to finish it than in a series of symmetrical units where the movement is not back and forth, but balanced and resting on the central point. In other words, in Group I there is a rhythm of movement within the group itself, as well as of the whole, while in II it is balanced and coördinated from the centre of each group, out and back, so that a longer, or at least more important end of some description is necessary to break the rhythm, and stop the series in I than in II.

It is noticeable also that H. and S. thought in both cases they were making the end spaces equal to the interspaces; but after Series I, *a* was made 102 and 109; and after Series II 95 and 104 respectively. This naturally raised another question: Does a series of groups, with repetitions within each, tend to make one overestimate distances between or at the end, or at least does one overestimate these distances in comparison with a series of symmetrical units?

The subjects were so unanimous in preferring a larger "embankment" after Series I than II, that it was useless to test them further on that point, and the experiment was changed to the other question according to the suggestion above.

A series of eight cards was prepared (125 mm. wide) on four of which five heavy black strips were drawn equally distant from each other, and on the others a much wider strip in the centre with another on each side near the edges. Of the two series just made, one was composed of what we have called "rhythmic units" and the other of "symmetrical units."

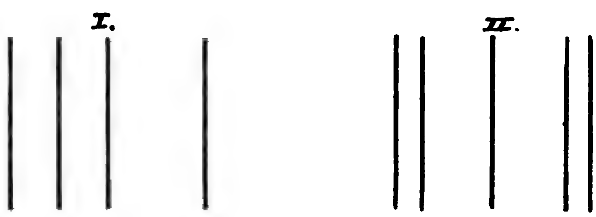


Fig. 25

The subjects were asked to arrange the two separate series so that the interspaces should be exactly equal to the units. It will be observed that the rhythmic unit had a black strip on each edge, thereby apparently decreasing its size, while the edge of the symmetrical unit was white. In this respect the comparison was hardly fair, but the result was the following. The figures represent an average of

two trials, and stand for the size estimate of the interspaces for each subjects respectively, in the two series.

	I	II
J.	132.5	130
S.	125	122
U.	133	129
H.	128	124
R.	126	122
W.	136	133
V.	129	131

Average difference of estimate of both series = 2.64. Mean variation = 1.37.

It might be contended, however, that Series I is an example of optical illusion, that the card was overestimated for that reason, and the interspaces necessarily made wider. To avoid this difficulty another series was made. Two sets of cards (125 mm. wide) were prepared; one with five black strips at equal distances apart as before (excepting that the strips were made heavier), the other with six strips. The card with an odd number of strips had thereby a strip in the middle upon which the attention could centre,—possessed a kind of balance. The card with an even number of strips had, moreover, no such central line but only a space, thus preventing repose of attention, and making the unit more pronouncedly rhythmic. (It will be noticed in the foregoing table that one subject, V, made narrower interspaces in I than in II. He said he felt the units as centring around the group of three lines in the centre, not as proceeding equally to the edge. The unit became thus for him symmetrical instead of rhythmic, which could easily account for the difference in estimation.)

The results in the present case are an average of three trials:

	I, 5 strips	II, 6 strips
J.	129	137
S.	125	129
U.	132	133
H.	125	131
R.	127	138
W.	126	133
V.	123	129

Average difference in estimate of both series = 6.1. Mean variation = 2.1.

Since both these figures represent an effect usually explained by optical illusion, that factor may be counted out, and the difference

in the estimate be accounted for by the difference in the rhythm of the units. The difference in estimation between the two rhythmic units, differing only in odd and even number of strips, is greater than between the rhythmic and more strictly symmetrical, and yet the two were more comparable in construction. It would seem, then, that the greater overestimation of II is due to the rhythmic movement which is not limited or driven back to a central line as in I, but, by continuing over the limits, produces a greater feeling of breadth.

The same question was experimented on in another way. Smaller strips of cardboard all 50 mm. wide, but with different designs, were hung behind the narrow window previously used: Four of each set were hung at a time behind the window, and subjects arranged them so that the interspaces appeared to equal the strips. These designs were to illustrate different points in question. The difference in estimation for an empty card, and a filled one; the difference according to the strongly centred, or rhythmic, or slant lines of the filling. These experiments were not so complete as the former ones, since the subjects were scattered; hence they represent only one trial or an average of two. But the results conform with what we have been led to expect.

1 2 3 4 5

Fig. 26

	I	II	III	IV	V
J.	49	57	52.5	52	53
S.	51	56	50	49	51
U.	53	54	49	50	48
H.		53	52	49	51
R.	52	52	51.5	48	51
W.	50	55	50	50	52
V.	51	53	49	48	51
Average =	51	54	50.6	49.4	51
Mean Variation =	1.5	1.4	1.2	1.06	.9

These results point to the fact that there is a tendency to overestimate the strips unless there is a strong central accent, which draws the attention back to the middle of the strip, in which case it is slightly underestimated. This would seem to be contradicted in I,

where the centre is strongly marked by slant lines coming toward it. But the subjects, instead of taking the lines as pointing *towards* the centre, in almost every case felt them as leading *away* from it, and the oblique lines gave an appearance of greater breadth, which result was carried out by the greater overestimation of II. In this case, in addition to slant lines, there was no central accent, and the overestimation was proportionately large. III and IV were intended to illustrate the difference in estimate of rhythmic and symmetrical units, but although a slight difference is apparent, the subjects did not feel III as strongly rhythmic, because the black lines on the ends of the strip were ignored against the black background, and only the two central lines were taken. This made it more a balanced than a rhythmic unit, so it is not a fair type of the point in question.

We may say, in conclusion, that oblique lines (which involve a more complex muscular adjustment to perceive them) give an impression of greater distance traversed, hence are overestimated; of two rhythmic groups, the one containing an even number is more overestimated than the odd, since the movement across is unchecked, and not balanced around a central line; a series of strongly centred groups is more correctly estimated as to its interspacing, and even slightly underestimated, because of the check imposed by the centre of fixation in each group. Although these results are very uniform, a more complete series of experiments should be done on this subject, to make the conclusions thoroughly valid.

Another question was suggested by these results: Is it more agreeable to have a series of repeated space forms nearer or farther apart when a design is within? Does the design, by drawing attention to itself (especially if it be markedly central), make the objects demand narrower or wider interspacing? To test this question, four blank strips of cardboard were hung behind the narrow window, and the subjects arranged them at the distance apart which suited them best. Then two other sets of cards, of the same size, but of different designs, were hung successively the same way, and these arranged also at the most agreeable distances. One decorated card had a circle within a rectangle, the other a triangle of gilt stars. The judgments were made in pairs, *i. e.* the blank cards and the one with the circular design were arranged twice in succession; then the blank cards and the star design. This gives

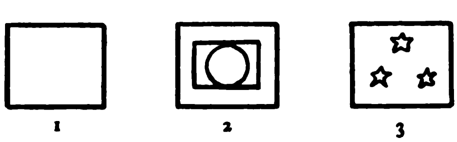


Fig. 27

three judgments for the blank cards, two for the circular, and one for the star design, and the judgments are given in full, since an average would disguise the point in question.

	I	II	III
J.	50	50	
	55	52	
			50
S.		70	
		20	
			35
		75	
			65
		45	
		48	
			50
		25	
		10	
			23
		35	
			65
		10	
		10	
			0
		60	
		35	
			25

Although the favorite arrangements varied somewhat on the different days, the *filled* cards were with only four exceptions preferred nearer together than the *empty* ones at any one trial, and two of these put them equal. (The choice of V. was affected by the fact that the circular designs produced such strong after-images that he was obliged to put them farther apart, to avoid confusion with the real design.) The reason suggested by the subjects for a narrower interspacing with decorated cards was that, when they attended to the design, they paid no attention to the actual edge of the card, but the card ended so far as its interest was concerned with the design. Therefore they had to be nearer together to bring the *designs*, not the whole field of the cards, into a series. If, then, the design extended over all the card, and its interest was no more in the centre than the ends, would this difference in interspacing cease to be demanded?

Another series of cards was hung with a design of oblique lines over the whole field, and these arranged as the others were, at the most agreeable distances.

	I		IV
J.	53 mm.	J.	53 mm.
S.	45	S.	47
U.	52	U.	50
R.	35	R.	37
W.	23	W.	43
V.	35	V.	34

If, for the sake of comparison, the average be taken of the favorite arrangements of the four blank cards, and they compared with the interspacing of the oblique line design, it will be seen they approach each other closely, except in the case of W.

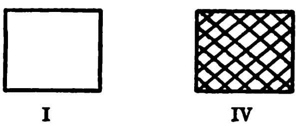


Fig. 28

These experiments would seem to show that an empty space, or one completely covered with decoration, is taken in its entirety when repeated in the series. But when decorated, especially toward the centre, the *design*, instead of the whole including space, is taken as the repeated unit, and for this reason the different units must approach each other to make a satisfactory series.

To what extent does change in *level* and *plane* affect the units of a series? To test this, a series of diamond shapes was hung on the same level and at equal distances, and the subjects enjoyed them as a repeated series.

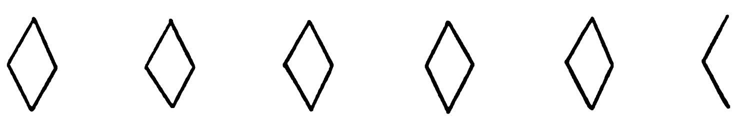


Fig. 29

Then another row was hung above them, and halfway between.

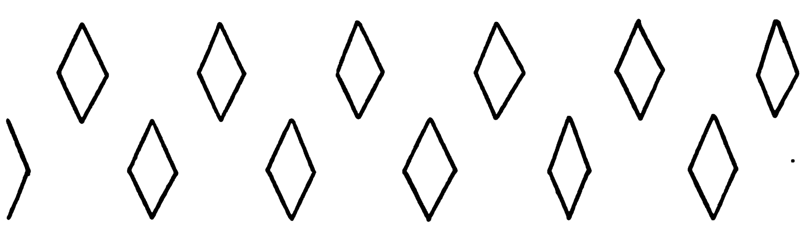


Fig. 30

The subjects grouped them either in twos or threes, thus transforming them into one series of similar group-units of triplets and pairs. They were asked if they could take them up and down, one

after the other without grouping, as they would have done when on the same level. With a little practice two of them succeeded, but they found the series tiresome when taken in this way, and deprived of much of its pleasure.

The series was then changed by hanging a smaller diamond between the others, at the same level.

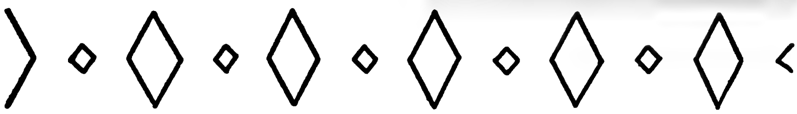


Fig. 31 A

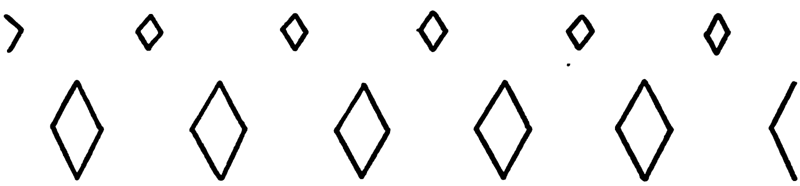


Fig. 31 B

This was enjoyed even more than the other, as an alternating series, but when the smaller diamond was hung between but on a higher level although it could still be included if *grouped* in some way with one or two of the larger diamonds, it baffled all attempts to include it as an alternate minor unit in the other series. The two series separated, and one ran along above the other, or else a definite grouping took place, so that the large and small diamonds made one group-unit which was repeated. But to combine two different elements as major and minor units of one series, when the two were on different *levels*, was generally declared impossible.

Provided units stay on the same level, however, a difference in *plane* does not prevent their being in one series, provided the plane varies regularly, and the variation is not too great. The variation in plane of a few inches, used with these shapes, did not prevent their being taken as one series, although it much facilitated their being taken as two, if desired.

These experiments have all pointed to the fact that our pleasure in repeated space forms is an immediate experience. We do not look over the series and merely recognize that regular repetitions occur, but there is an immediate *feeling* of repetition, analogous to our feeling of auditory rhythm. This feeling does not always accom-

pany a recognition that certain repetitions occur, but is a distinct experience in itself dependent upon certain conditions in the series. The series excites a certain response in the observer, which, if it corresponds with his rhythmic organization, is pleasant, and if not, is otherwise.

With a certain class of subjects this rhythmic response is very noticeable, and they feel it as a conscious part of the experience. With others, the symmetrical properties of the series are the more prominent, and they detect no rhythmic response until the necessary objective conditions for such a rhythm are violated. Then they feel it as keenly as the other type.

In a series of units, there is a rhythmic discharge of energy on each, the fixed temporal intervals being regulated by the alternating space. When the units are too near together, or when the alternating spaces vary irregularly in size, this rhythm is disturbed. If the alternating spaces vary regularly in size, a richer rhythm is introduced, which increases the pleasure up to a certain point when more variation makes it too complicated, and confusion results. When one element alternates with another, the one on which more energy is expended for any reason becomes the principal unit. The other has less significance as to its content than as to its size, for on this depends the regularity of the rhythm. Variations in the content of alternating figures must be cautious not to disturb, by the extra attention necessary to take them in, the rhythm of the whole. Variation in the principal unit may take place almost without limit, provided there is an equal amount of interest in each, thus making a rhythm of equal discharges. There must be an alternation of *two*, *i. e.*, of discharge of attention and rest. However rich the rhythm is made by greater and lesser accents or groupings, the rhythm must fall eventually into a discharge of attention, and a rest-period. In the temporal type of subject, to whom the actual *motion* across the series is a necessary factor of the pleasure, this discharge and rest-period of *attention* is exactly inverse to the *motion* across the alternate and rest upon the unit. That is, on the principal unit is the discharge of the attention, but the rest-period of the motion across; while the alternate unit supplies the field which is travelled over, but requires but little attention.

The rhythm of the series may be not only of the units, but inside the units as well, in groups of elements. The rhythm inside such a group may be of two kinds: (1) a rhythm, which is at the same time restrained and coördinated about a central point or line, and (2) a

rhythm which goes back and forth from edge to edge, and has a tendency to overstep its limits, thereby carrying on the series with more activity. The former is more connected with odd-number groups, and the latter with even, although modifications in their arrangement may reverse the effect. Since the eye moves more slowly and intricately over a curve than over a straight line,[1] it may be that that is the reason why an arched series is taken as the unit of a series, rather than the vertical supports (as it invariably is in some unfinished experiments not recorded here), whereas in a series of lintels the horizontal line of the lintels requires less muscular adjustment to perceive it than the vertical support, therefore the latter are taken as the units.

In any case, the unit of the series which attracts the most attention and interest, for whatever reason, is taken as the principal unit, and may vary in details, while the alternate must be invariable, except in certain cases where it meets other demands. There may be rhythm in a series, and at the same time symmetry with respect to a middle point. In such a case a balance must be obtained between the two different motor responses.

A series of analogies between the rhythm of sounds and of visual objects, will illustrate more forcibly the similar demands of both.

(1) Auditory Rhythm: Periodicity is necessary. Accentuation may be supplied by the subject, but there must be fixed temporal intervals, and if the temporal conditions are not fulfilled, no impression of rhythm is possible.[2]

Visual Repetition: Alternate spaces must be of invariable size, or the series is broken up. Different degrees of interest may attach to the principal unit, or the subject may group them in different ways, but the alternate spaces must be uniform, or there is no feeling of rhythmic repetition.

(2) Auditory Rhythm: Sounds must recur within a certain rate. When succession falls below, or rises above a certain rate, no impression of rhythm results.[3] A certain voluminousness is necessary for very slow measures, to make the separate elements connect themselves in a series.[4]

[1] Stratton: Eye-Movements and Æsthetics of Visual Forms, Philosophische Studien, vol. 20, p. 350, 1902.

[2] MacDougall: Structure of Simple Rhythm Forms, Harvard Psych. Studies, vol. 1, p. 321, 1903.

[3] MacDougall: Structure of Simple Rhythm Forms, *ibid.* p. 322.

[4] MacDougall: Structure of Simple Rhythm Forms, *ibid.* p. 322.

Visual Repetition: Objects must recur at certain proportionate distances from each other, to connect themselves into a series. Larger objects may be at a greater distance from each other than smaller ones, and still form a series.

(3) Auditory Rhythm: "Perception of rhythm is an impression, an immediate affection of consciousness, depending on a particular kind of sensory experience. It is never a construction or reflective perception that certain relations of intensity, duration, etc., do obtain." [1]

Visual Repetition: The feeling of rhythm in a visual series is immediate, and wholly distinct from the knowledge that certain objects do recur. This is especially illustrated in repetitions of three distinct units, when subjects saw and understood the scheme of repetition, but could not feel it.

(4) The number of units in an auditory group depends on the rate of succession, but any higher number of elements in a group than *six* or *eight* falls back into smaller groups.[2] *Eight* is about the highest number that can be held in a rhythmic group.[3]

Visual Repetition: *Eight* is the highest number that can be held in a rhythmic group, and some subjects can only hold seven. Many more units can be felt in a group, when the size of the including space is taken as the measure and compared; but no more than eight can be felt and recognized as the number of units it is. (There may be exceptions to this rule in either auditory or visual rhythm, but this is the norm in both cases.)

(5) Auditory Rhythm: In all long series, there is a subordination of the higher rhythmic quantities, so that opposition of simple alternate phases tends more and more to predominate over triplicated structures.[4]

Visual Repetition: However complicated the repetition becomes, with regular variations of the size of major or alternate units, the units tend to re-group themselves, and so resolve ultimately into a simple alternate repetition of two group-elements.

(6) Auditory Rhythm: "The introduction of variations in the figure of a group does not in any way affect the sense of equivalence between the unlike units." [5]

[1] MacDougall: Structure of Simple Rhythm Forms, p. 325.
[2] Bolton: American Journal of Psychology, p. 223, 1894.
[3] Külpe: Outlines of Psychology, p. 395.
[4] MacDougall: Structure of Simple Rhythm Forms, p. 348.
[5] MacDougall: Structure of Simple Rhythm Forms, p. 349.

Visual Repetition: Changes in the content of the major unit do not affect the repetition provided the alternate space remains invariable.

(7) "Feeling of rhythm is more definite as we proceed in a verse, or in a series of simple sound sensations. At first the cycle is not perfectly adjusted, and complete automatism established." [1]

Visual Repetition: Observers often had to look over a series several times "to learn it" as they said, before the rhythm was felt.

To these may be added several other analogies, which, owing to the fact that visually repeated objects remain in the field, while auditory rhythm is purely successive, have other features which somewhat confuse the resemblance. The principle, however, is the same in both.

(8) Auditory Rhythm: "At the close of a period, we have a pause, during which the tension between the two opposing muscle-sets dies out, and we have a feeling of finality." [2]

Visual Repetition: An alternating series must end on the heavy unit, but since one does not look at series from left to right any more than from right to left, a heavy unit must be at both ends, not on one alone. In auditory rhythm, this final pause is not a function of any of the intervals of the period, for it comes at the end, when the sounds are no more present. But in visual repetition, after feeling the series rhythmically, it is still in the field, either as an unending series, or as a whole, in which each part is equally related to all the others. The final pause of a series that ends must be at *each* end, and the series perfect from either point of view. It therefore fulfils the demands of symmetry as well as rhythm, but since symmetry in its strict sense has no meaning for sound-series, this double finality of visual repetition cannot be analogued in auditory rhythm.

(9) I have found no recorded experiments of rhythms of sounds of different timbre and pitch, *i. e.*, a regular rhythm of a bell with a violin, a piano with a whistle, etc. It would seem, however, that such a succession would produce the same irritation as a visual repetition of incongruous elements; as a circle introduced into the Greek fret, or a series of Renaissance columns and Egyptian monoliths. In both cases, the whole set of adjustments for each element would be thrown into confusion by encountering the next one, which would require a different attitude. Such a readjustment would be impossible in the

[1] Stetson: Rhythm and Rhyme, Harvard Psychological Studies, vol. 1, p. 455, 1903.

[2] Stetson: Rhythm and Rhyme, Harvard Psychological Studies, vol. 1, p. 455, 1903.

space necessary for the perception of any rhythm, hence there must be congruity in elements, either auditory or visual, to be in a series at all.

(10) Auditory Rhythm: "If every alternate element of a temporally uniform sound-series receive increased stress, the interval which succeeds the unaccented sound will appear of greater duration."[1]

Visual Repetition: The distance between unit groups with a strongly accented centre appeared shorter than between rhythmic groups where the movement was not restrained at the centre, but went from edge to edge. The principles which explain these similarities are, however, different.

In the auditory rhythm, the stronger accented beat excites a greater response than the unaccented. This lasts over longer in consciousness, and for that reason the interval after the accent seems shorter.

In visual rhythm, however, the symmetrically rhythmic group drives the attention in toward the centre, and whatever excursion it makes to either side, it returns finally to the centre. In the even-number rhythmic group, there is no such central line to restrain it, and as one goes across it one has less check at the edge, the rhythm does not wholly finish, and the space is thereby overestimated. The *overestimation* is due to the rhythmic activity in the group which oversteps its limit.

The essentially rhythmic character of the experience is, however, the same in both. The experience of visual repetition is only one-sided when the symmetry or proportion of a finished series is regarded as the explanation of its essential character, and when the temporal rhythmic factor is neglected.

Part II

The purpose of the latter half of this discussion of repetition is to consider a certain number of examples of its use in typical buildings of all the European styles of architecture from Greece down, and to show that the principles laid down in the earlier half have been expressed almost without exception in those of recognized merit. In other words it is to show that the laws of repetition, which have been brought out in the experiments of the first part, and which would of necessity be true if that explanation were correct, have indeed been exemplified in types of architecture universally accepted as beautiful.

[1] MacDougall: Structure of Simple Rhythm Forms, p. 377.

The illustrations have all been drawn from architecture beginning with Greece, and not from the older Eastern styles. Egyptian architecture, although it recognized the importance of repetition to some extent, in its colonnades, avenues of sphinxes, and hieroglyphic decoration, never reduced it to any principle, nor adhered to any one scheme throughout a piece of work. Supports of the same kind and diameter have no fixed relation to each other, they may be of the same or different lengths, and may vary in diameter as well.[1] Spaces between columns of one size and design may vary considerably, and the entablatures be of different proportions. The art of Egypt was not rhythmic.

The architecture of Assyria and Chaldea had even less of repeated forms in its style. They made but little use of columns or piers, and had few arches.[2] The bare Assyrian edifice was like a great box, perpendicular to its foundations, and the long walls pierced by hardly an opening in the way of windows or doors.

Persian architecture was noted for extreme nicety of execution, but a monotony in all its forms, and conventionality about its use of the column, which makes it little more fruitful for our study of repetition as an artistic value. In its decorations of bas-relief, the pose and gesture of each figure is so exactly similar that they appear almost machine-made.[3] When a little variety is introduced, it is evidently done with misgivings, and shows none of the spontaneity or first-hand pleasure in either repetition or variation which would make it profitable for illustration.

Such a lack of feeling for repetition is, indeed, according to the peculiar genius of these styles of architecture, what might have been expected. The ruling idea, especially in Egyptian and Assyrian architecture, was ponderous strength. Everything was built with the idea of remaining immoveable through centuries to come. The enormous temples and tombs, the long palaces with their heavy walls without an opening to relieve them, the pyramids themselves like mountains of rock — all these meant strength and immutability, to which the motion and rhythm involved in repetition was totally foreign in spirit. In Persia indeed (as well as India and China, which will not be considered here) there was a change in tone. The column was used, not the massive one of Egypt, but a lighter

[1] Perrot and Chipiez: Art in Ancient Egypt, p. 100.
[2] Perrot and Chipiez: Art in Assyria and Chaldea, p. 126.
[3] Perrot and Chipiez: Art in Persia.

shaft, which showed a tendency toward other effects than immensity and strength.

With this change of ideal, repetition in some kind of system made its appearance, but its variations were tentative. It had not become used to its new sense, and it was left for Greece to develop the rhythm and movement of repetition, and to combine it with proportion and symmetry into its perfection.

The method of analysis employed has been to go through a certain number of architectural photographs, picking out all the examples of repeated forms of any description, and classifying them according to the principles which they exemplified or seemed to violate. For this purpose a collection of about five thousand photographs from the library of Robinson Hall, Harvard University, was analyzed. The photographs were taken in order of styles: Greek, Roman, Romanesque, Gothic, Italian and French Renaissance, and modern. The examples of the different points in question were taken as they came in the cataloguing of the library stacks, without respect to whether they appeared to bear out the previous conclusions or not.

Variation of Alternating Units

The first principle which we shall consider is the variation allowable in the units of an alternating series. It will be remembered that the principle was as follows: (1) In any series of two alternating units, the one on which the most energy is expended is regarded as the principal unit, the less important one as an alternate. Variation of the principal unit is allowable, often desirable and even necessary; variation of the alternate never allowable, unless other circumstances change the situation. If the minor unit is changed, so that in interest it equals the major unit, the rest-phase of the rhythm is destroyed, the effect is of two rival repetitions going along together, and fatigue results. If variation in the alternates exceeded that of the principal units, the balance of the rhythm would change, the alternate become the major unit, and a new series begin.

From the very nature of the case, then, it will be impossible to look for variations in alternates, which make it exceed the principal units in interest. We must investigate alternating series, in order to see if *one of the elements remains the same*, while the other may or may not vary. If this were true, a rest-phase for the rhythm

would be assured in the series, while the principal unit might vary, provided the same amount of attention were required in each case. (2) It will also be remembered that *size* and *limiting shape* were the factors that could not vary without doing violence to the rhythm, while content might vary almost without restriction. (3) The position of alternating units as regards each other cannot vary; the two units are so dependent on each other that the position of one must remain halfway between two of the opposite kind. In other words, if the two series of units run between each other, they form *one* series or rhythm. Two rhythms cannot be kept up alongside; so if one unit, however regularly placed with regard to another of its own kind, recurs at unequal distances from the *other* units, the feeling of the repetition is lost, the rhythm broken, unless the two units can be grouped into one, and so make a single rhythm again.

We shall, then, look for alternating series, of which the two units are at *equal* and *invariable* distances from each other; the variations of content (if such there are) occur only in the major unit; and are of the filling, not of the including shape or size.

It may be readily seen that there are difficulties in finding alternating series which exactly illustrate this particular point, or in reducing them to any system. It was necessary to look through many photographs to find one that presented the required conditions (*i. e.*, two repeated series of units, alternating with each other), and when found, they were of so many different varieties, from windows in an apse to reliefs on a fountain, that each has had to be described by itself, and any rigid classification was impossible. Moreover, it was difficult to find a scale of judgment by which to decide whether a series was really alternating or plain repetition. From one point of view, *every* repetition is alternating, that is, the repeated unit always alternates with an empty space. Although such repetitions bear out the theory still further, and emphasize yet more strongly the invariability of alternates, and the possibility of variations in the principal units, I have used the term in a stricter sense, and only given illustrations of repeated objects, when one unit actually alternated with another definite unit.

Had the other sense of the term been used, examples might have been multiplied without limit, of slightly varying repeated units, and unvarying alternate blank spaces. But it was felt that such accumulation of illustration was unnecessary, and that what was true in a stricter sense of the term would be recognized as true for the larger number of cases that might be cited with a wider meaning.

If the minor units had a definite enclosing outline, they were counted even though they were blank within, but without an enclosing outline, that is, if they were mere spaces, they were not considered, although the fact that there is such universal use of this type of decoration shows only more conclusively how the necessity of the invariability of alternates is taken for granted as an axiom of design.

Another type of alternate repetition was not included in the illustrations, *i. e.*, when two sets of units alternated, *without variation in either one.* To this class belong all the conventionalized designs used so much in all kinds of decoration, and of which a very full account is given in Owen Jones's Grammar of Ornament.

These, to be sure, illustrate the negative points, viz., that size and shape are unalterable for rhythmic repetition; that distances must be equal and invariable; and that alternate units must not vary. But since the principal units do not vary either, it seemed needless to give them as examples of the point in question. A mention of this class of alternating repetitions, of which there is such a great number, is enough to show that they fall within the theory. But one example is as good as a thousand, and their inclusion among the illustrations for rhythmic alternates will be taken for granted without further mention.

We are left, then, to the consideration of those alternating repetitions alone, where both have a definite outline, and one or both varies to a greater or lesser extent. The effort will be to show that the unit which for some reason is of principal importance in the rhythm, is the one chosen to vary, and if not that the repetition suffers thereby.

125 Examples

A. Variations in Principal Unit alone: 87.

I. Content alone:

a. Metopes and Tryglyphs in Friezes: 9.
b. Arches and Columns: 2.
c. Statues in niches alternating with supports: 37.
d. Windows alternating with supports or decorations: 12.
e. Paintings or mosaics: 7.
f. Carved designs in screens or ceilings: 17.

II. Size and Content.
a. Doors, paintings and reliefs on façades.
Vary in size to emphasize symmetry: 4.

b. Statues or shields over arcades.
Vary in size to complicate rhythm: 1.

B. Variations in Principal Unit AND Alternate: 37.

I. Content alone:

a. Windows and decorations on façades.
Alternates vary in design to emphasize symmetry: 4.
b. Windows and turrets. Vary alternately in design to complicate rhythm: 2.
c. Reliefs alternating with tablets; reliefs or statues and pillars: Alternates vary in design to give richer effect: 11.
d. Alternate unit is human figure: 9.
e. Alternate unit varies in design, but is on a different level: 3.
f. Irregular variation in alternates (windows, shields, and railings): 3.

II. Of Size of Alternate Unit.

a. Windows and supports. Vary in size to emphasize symmetry: 1.
b. Statues and pillars; windows and pilasters.
Alternates vary in size to complicate rhythm: 3.
c. Vary in size irregularly. Disorder: 1.

C. Variation of Distance.

Row of windows — Distance between first two is wider: 1.

The 125 illustrations of alternating repetition which were taken at random among 5000 photographs show a decided compliance with the principles already laid down. But there are many divergences as well, which it is necessary to consider, to see whether they are really contrary in principle or fall under its wider application. Eighty-two accord exactly with the principles with which we started. The distances between each set of units are equal and invariable; one unit varies in content but not in size or including shape; the alternating unit is invariable.

There is an interesting modification of this principle in the case of the metopes and triglyphs of the Greek friezes. Here the triglyphs are unquestionably the principal units structurally, and to many observers the principal beat of the rhythm when taken rhythmically. But the triglyphs never vary and the metopes do, which would seem at first to violate the rule that principal units alone, and not alternates, should vary. This difficulty is obviated in two ways. With the spatial type of observer, the triglyph is indeed the principal beat

of the rhythm when the series is at such a distance that the difference in the metopes (if there is such) cannot be detected. When, however, the series is nearer at hand, there ceases to be any rhythm, but each carved relief is taken for itself without regard to the others. With the rhythmic type of observer, if the triglyph has been the principal unit before, the principal beat changes on nearer approach to the metope and the whole series shifts its accent. It is impossible for any observer to keep the triglyph as the principal unit of the rhythm, when so near that differences in the metope are easily perceived.

There are still thirty-eight cases which vary from these rules, and many of them vary in more than one respect. These exceptions fall into several classes, quite distinctly marked off from one another, and will be taken up in turn.

In five cases, the *size* of the principal unit varies as well as the content, but in four cases the variation of size is either at each end, or in the centre unit, to emphasize bilateral symmetry of the series as a whole. The series in this case is taken as a larger unity of which the separate units are parts; and hence they are not only repeated with respect to themselves, but are symmetrical with respect to the whole. In the other case where the size of the principal unit varies, it varies on *every other one*, thereby complicating but not confusing the rhythm, *i. e.*, a stronger accent comes on every other principal unit.

There are also five cases in which the alternate spaces vary in size. Three vary regularly, thereby enriching the rhythm by introducing alternate heavy beats, and one varies at each end of the series to emphasize bilateral symmetry of the whole, with regard to the central unit. In the other case the alternates vary in both shape and size, with no regularity and from the point of view of repetition alone, disorder is all that results. This is on the Palazzo Pretoria in Pistoia, where carved shields occur at equal distances between windows. These shields are not component parts of the building, but were added with some other kind of significance; hence they express nothing so far as repetition for its own sake is concerned.

The other variations are all in the *content* of the alternate, minor space. Four vary *symmetrically* in the designs on each side of the central point, so as to accent the bilateral symmetry of the whole taken as a unity. Two vary rhythmically in design, *i. e.*, there are two sets of designs which alternate with each other in the unaccented spaces. When they vary regularly in design, the rhythm of the whole is *enriched* not confused, provided there are only *two* sets, not

three or more. The alternate spaces are passed over on the way to the principal unit, but by having an alternating design between them (varying only in detail, but of the same general character) a more complex rhythm is introduced which is good, since in both cases the alternates and principal units are so different they could not possibly be confused with each other, even though both varied. (In one case, turrets and statues vary with windows of the same shape but different decorations; in the other, arched windows and arched spaces alternate with statues.)

Eleven more cases of variation in the minor as well as major spaces fall under another head. These do not vary with regularity, but are different in each case — the detail of the design varying, while the shape, size, and distance remain unchanging. It is interesting to notice that these examples of variations of alternates were almost all taken from examples of Renaissance architecture, where a richness of effect was desired, even at the expense of regular rhythm. This could, indeed, be attained in no other way so well as this. In all these eleven cases, the conditions are alike: both of the repeated units are enclosed by limiting lines of unchanging outline. The principal units are more prominent than the others on account of greater size or interest, but the alternates, instead of retiring entirely into the background, have slight variations in decoration. This variation is always only in detail: the tracery on the pillars of the tomb of Louis XII, of the Loggia dei Novoli, in the Chiesi di Frari, Venice, etc. So unimportant in fact is the variation that it is not observed until one attends closely to it, and yet the rhythm is just enough disturbed by its presence to give a feeling of *extra sensation* or *luxuriance* which cannot be attained through variation of the major units alone. It is in the alternate spaces that the *feeling* of repetition lies. Any material change in them destroys the series, but a slight variation in the lines of decoration, a little rearrangement of the conventional curves in each alternate, gives, even though unattended to, in fact partly *because* unattended to, a vague feeling of variety, of some superfluous sensation being brought into consciousness, although the regular shape, size, and distance of the objects remains unchanged. It is this feeling of superfluity and slight disturbance which constitutes the peculiar richness of certain styles. These examples, then, far from falling outside of the laws of repetition, owe their opulence of sensations to the very principles of regular rhythm which they violate.

Another set of exceptions will involve more searching analysis.

Nine of the examples described have the human form for the alternate unit, and in every case where this happens, the alternate varies. In the majority of cases where statues of the human form alternate with any other object, the statue is taken as the principal unit on account of its superior interest, but this is not always the case. In the Padua Basilica, and in the Church of St. Guistiana, cherubs alternate with conventional decorations, but the latter are so much larger and more elaborate that they would naturally be taken as the principal units. In the other seven cases, statues alternate with bas-reliefs which also have human figures in them; hence, since the bas-reliefs equal the statues in interest and exceed them in size and importance, they are taken as principal units.

It might at first be expected from the previous discussion that, in order not to shatter the repetition, the alternate statues must be alike, must be conventionalized into identity; but this is not the case. Another principle now comes into play. We demand variation in the human form whatever its place in art, even in the unimportant position of alternate in a repetition, and although they are kept as much alike in pose, size, level of head and feet, general character (*i. e.*, cherubs do not alternate with old men, nor draped figures with undraped), yet there is some variation of pose or direction of glance, to keep them from being duplicates. We should expect this variety of repetition to be in danger of becoming fatiguing because of its lack of an unchanging rest-phase, but this difficulty was evidently felt in building them, for in every case *some unchanging element* has been supplied to the series to bind it together and to keep the constant changes of attention from upsetting the series. The cherubs of the Padua Basilica are in high relief against a uniform rectangular background which does not vary, and which furnishes an alternate just in character with the principal unit, the bas-relief. In the Cantoria of Donatello, although the dancing children move across the whole space, uniform double columns occur at intervals, and supply an unchanging alternate, while the children vary in position behind them. Around the pulpit of Lincoln Cathedral, although both units, reliefs, and statues vary, the pilasters behind the statues are invariable and supply a constant, unchanging factor in the series. In the alternating reliefs and statues of the Milan Cathedral or in the paintings of different sizes in All Souls Church, Oxford, an unchanging element is supplied in the frame, which is of like design in every case, so that in passing from one to the other an unvarying alternate is always present. In the Sienna font, and in the statue to

Leonardo da Vinci, which are types of a vast quantity of repeated forms, there is uniformity in the minor pedestals and in the frames of the alternating bas-reliefs, which supplies the unchanging factor.

Moreover, another factor is noticeable in this kind of repeated series, — it is never long. The fatigue which would certainly result from a too long continuance of varied alternates, even with unvarying factors in the way of supports, pillars, and frames, is obviated in various ways. The series is either short and the whole has a definite bilateral symmetry, as in the Padua Basilica, and in the Oxford church; or, as in a great number of cases, the series goes around a fixed central point so that only three units are seen at a time. It is thus especially that this method is used in fonts, pulpits, and monuments, where from the circular arrangement enough can never be brought into the field at once to fatigue the attention.

This consideration of alternates which vary widely, as do human figures, even when they are alike in size, general shape, and character, and, moreover, the discovery that there is almost without exception an invariable element *between* the other alternating units, *i. e.*, a third alternate; or *behind* them, as in the case of the pilaster behind the statue, may well bring up two questions:

(1) When the unchanging factor comes *between* the other two units, is not *it* in reality the alternate, and the two other units either variations of shape and size of one principal unit or two sets of principal units? In other words, do we not actually apperceive the two principal objects as the units of importance, and take the unvarying factor which comes between, no matter how slight it may be, as the alternate? Do we not demand the unvarying as our alternate, no matter how many variations may be in the other figures?

(2) When the unchanging factor comes *behind* the alternating statue, in the same plane with the bas-relief, do we not inevitably take *it* as the alternate in the series, and regard the statues more as episodes or attendants on the series but with real values of their own? Is not the fact that the unvarying factor and principal units are in the same plane an indication that they constitute a real series, while the statues or paintings which are in a plane by themselves make a series, harmonizing with the other, it is true, and in part coinciding with it, but felt in a different way? Therefore the actual repeated series conforms to the given conditions and is made to do so in every case by its unchanging alternate in the same plane; while human figures with values of their own never can be considered quite as alternates, but are really felt to be a series by themselves.

This introduces another question. In two more cases of varying alternates, there was variation in decoration above the level of the rest of the series. In the Borghese Casino, there is variation in the busts placed *over* the alternate windows. In the Venice rood-screens, there is variation in the carving of the alternating supports, which rise *above* the rest of the series. Is that part of the series above the level of the principal units really included in its perception? It would seem rather that when the series as a whole is being taken, those variations above the level of the main units (if they are not very marked, and they were not in either of these two cases) are ignored or only felt in a vague way as added richness. When, however, the attention is turned toward them especially, they form a series of their own, in which they become the principal units, and alternate with empty spaces. There is no limit to the changes possible in apperception, according to the *level* and *plane* of the alternating units.

There are three cases left; two where alternates vary in content with no system, and one with variation in distance. The first two are differently carved sections of railing on the side of Freiburg Cathedral, and a differently decorated frieze of squares and circles in the S. Lorenzo Cloisters, Rome. The effect is only of disconnected and fragmentary series in both cases, and especially in the latter case it is impossible to feel it as a repetition at all unless the variations are ignored, and the attention fixed on the unvarying factor of size.

The variation of distance is in the Beauvais Palais de Justice, where the first window is at an unequal distance from the others in the series. The effect is only of disorder and accident.

We have, then, surveyed all our examples of alternate repetition, and found that in the exceptions to the general principles laid down some other effect than repetition as such was sought. Either (1) symmetry for the series as a unity was required, which demanded variation of the end or central units. In so far, then, as it fulfilled the requirements of symmetry, those of repetition were disregarded.

(2) Richness of effect was accomplished by those slight variations in decoration of alternates as well as the principal units. These by their vague suggestions of different combinations of similar elements, and minor differences felt but not attended to, gave a superfluity of experience which made up its peculiar richness.

(3) When the human form (or any other form of especial meaning in itself) makes the alternate unit, some variation is demanded as in keeping with its own significance, since in proportion as a thing has meaning in itself, it must not be exactly duplicated. But an in-

variable alternate is always supplied in the way of a frame, or background, which is felt as the real rest-phase of the rhythm, while the varying alternate forms have a place in a series of their own. Also, since such a complex attitude would be fatiguing, such series are always short, or circular, so that few units are in the field at once.

(4) Regular variations in size or content, in either major or minor which recur at fixed intervals, give a heightened rhythmical effect by making certain beats heavier than the rest. As has been stated before, the major unit holds within it the real significance of the *content* of the experience; the minor unit holds the secret of the *rhythmic* effect.

(5) Only 4 examples of the 125 were found to repeat themselves alternately with irregular variation of alternates and violation of the other principles laid down at the start. These can only be regarded as accidents, as faulty examples of art, whose virtue lies in some other part of the work as a whole, and not by any beauty they possess in themselves as repeated series.

Summary

125 *Illustrations.*

I. Variations in Principal Unit alone, 87		
	82 Content	
	5 Size	4 Symmetry
		1 Rhythm
II. Variations in Alternate Unit (and Principal Unit)		
	32 Content	11 Richness
		9 Human figure
		2 Rhythm
		4 Symmetry
		3 Different level
		3 Disorder
	5 Size	1 Symmetry
		3 Rhythm
		1 Disorder
III. Variation in Distance		
		1 Disorder

Several questions have been raised in this discussion of variations, but one which seems directly leading from it will be considered next.

When is variation *necessary* in a repeated series? We have considered the numerous cases where variation is *possible*, and the different ways in which a series may vary according to the idea to be expressed. Moreover, what appeared to be exceptions to the rule were

shown to be guided by a desire for some other effect than repetitions as such.

But when do we demand variation in a series? Is there any case where variation of the unit is not only allowable, but positively necessary to its æsthetic value?

There were no experiments on this question, for it will be seen from what follows that they would have been impracticable. But observation of several thousand photographs has made the following clear: When the series consists of objects having an æsthetic significance of their own, not depending on something else for their value, then variation is demanded. In other words, when a thing is an end in itself, we do not tolerate an *exact* duplicate. It may have a place in a series of others similar to it, but its own meaning loses force if another is beside it precisely alike. When, however, an object has no great significance by itself, or when however great its value, it be regarded as means to something greater, hence not an end in itself, it may be repeated without variation.

This principle may be stated from another point of view: Any work of art, of the *highest* significance in itself alone, must not be repeated at all. There must not be even the suggestion of repetition. The highest values are individual, and to have a copy or a series defeats its whole reason for being. Thus, a second Sistine Madonna, or a series of Venuses, would shock our whole æsthetic feeling. Moreover, we do not want a *suggestion* of repetition; even a series of different Madonnas in similar frames would take away from the significance of each, in so far as they were regarded as a series, and not as a mere collection of detached units.

But grading down from these works of the highest value in art, there comes a point where an object, although possessing considerable value in itself, is not so intensely individual but that it can gain somewhat by a place in a series of others like it *in some respects*, but differing enough so that each still keeps its own meaning distinct from the rest.

The balance between these two artistic aims, *i. e.*, the significance of the unit, and the rhythm of the series, must be adjusted with great nicety, and certain principles obtain wherever such series are found. It would be useless to cite the numberless cases where such series occur. Many have already been given in the examples of statues of saints, paintings on altar-pieces, and reliefs alternating with statues. One such series is a type of all. The human form represents that which has the most significance in itself, so when it is used in a rhyth-

mic series, its individuality must be toned down and conventionalized; it must have no marked feature in one unit that does not appear in another; the head and feet must be on the same level, or vary with regularity; the general character and spirit of all must be similar, but never identical.

The reducing to a common type is the demand of the rhythmic series; the difference in attitude and arrangement of detail is the demand of the unit.

Thus, the subjects chosen for repetition of this kind are in the majority of cases apostles and saints, whose spirit and general conception are the same; typical representations of abstract qualities, such as Virtue, Courage, etc.; or conventionalized cherubs, and even animals. As has been stated before, a long series of this kind is impossible without fatigue. In proportion as the object is repeated the individual units lose their own meaning, and they must have their individuality definitely toned down and conventionalized to avoid the clash between the two artistic values. Yet their essential peculiarity must always be maintained, for we refuse to admit or allow the total identity of any expression of living values, especially as expressed in the human form.

It may be urged that statues are often arranged at regular intervals around a building, where the effect of repetition is distinct, and yet each statue is distinctly valuable for itself. But a distinction must be insisted upon. The statues form a repeated series as regards uniformity in position, height, pedestal, and color, so that the direct sensuous effect may be called rhythmic. But as the attention fastens on each for itself and takes it for its own meaning, it ceases to be part of a series at all, but becomes a unit in a world of its own.

But what of the cases where the human form is repeated in a series, and does not vary? Examples of this are rare, but they do occur, and are interesting, since they throw light on what has been already said. In the whole collection of photographs only two were found where a series of identical statues of the human form occurred, — *The Porch of the Maidens* in the Erectheum of Athens, and the *Baths of the Forum* in Pompeii. In the former case the left knee of the caryatids on the right of the centre, and the right knee of those on the left of it, are raised a little; but aside from this slight variation the six statues are exactly alike. In the latter case a row of titans all around the interior bear the ceiling on their uplifted forearms and are all alike. These two examples are very perfect of their kind, and, far from offending us, are very satisfactory. The reason is

obvious. In both cases the statues are not the æsthetic end in themselves, but are there for a purpose, namely, that of a support. They are not ends but means to something else, and as soon as we feel *that* in regard to any work which would otherwise be of individual significance, it ceases to be individual, or to demand a peculiar expression different from all others, but may be duplicated without offence. Therefore, since the support of the superstructure obviously is dependent on the maidens in the one case and on the giants in the other, and since instead of existing simply for their own value they are there to hold up the roof, their artistic significance changes at once from *ends* to *means*, and variation is not required. Moreover, it will be found in the majority of cases that we demand this invariability in actual supports. Although we find but these two cases where caryatids are actually identical, we find also that in most cases the caryatids do not really uphold the weight, but a pillar or pier behind them supplies the real architectural support, and, that although they have a place in front of the pillar and give an apparent assistance in bearing the weight of the roof, yet the eye is not deceived. We see that the work is really done by the pillar behind them, so they that resume their place as artistic ends demanding variation, and not as means to something else. The following examples were found:

Milan. Arca di S. Pietro Martire. Pillars uphold the arch while four statues of women stand just in front. The pillars bear the weight although the statues add strength to the whole. The statues are varied.

Dijon. House of Caryatids. Piers behind the caryatids give real supports to the roof, while the figures added for decoration are all varied.

Dresden. Zwinger. Conventionalized figures ending at the waist are put on the outside of unvarying piers which bear the actual weight of the superstructure. The figures are all varied, but they cannot be conceived as really bearing the strain, since they have no foundation, but are merely added to the pier as a decoration.

Rouen. Tomb of Duc de Brezé. Four caryatids, all different, under four jutting projections of the arch. These projections are built securely into the rest of the structure and do not depend in the slightest on the figures for support. The figures are not integral parts of the whole architecturally, for the arch would stand exactly as well if they walked away, which indeed they are apparently in the act of doing.

Toulouse. Hotel de la Borde. Two caryatids under jutting projections of a window. The projections are securely built into the lintel and no weight rests on the caryatids nor even appears to. They are there solely as decorations and are different.

Paris. Hotel de Ville. Two caryatids under jutting projection of a window, again. Here is a very slight variation of the two female figures. The position of each is reversed to accent the symmetry of the whole. Very little weight is actually borne by them, but more than in the former cases, and we find proportionately less variation in the figures. They approach identity, but there is variation in detail.

These were the main instances found of the point in question, and are a type of the other minor ones found in support of pulpits, choir-stalls, and windows. It will be seen that in no case but the two classic ones given at the beginning are the human figures architecturally necessary to the structures, and in these cases they do not vary. In the other cases they are more or less playful, and the effect of the whole would be very unsteady did the superstructure actually depend upon them for support; but since piers rise invariably behind them and bear the weight, they fall into the sphere of decoration and from that point of view they must and do vary.

We have, then, considered variation of units in a repeated series, where they may vary and where they must, and we find the real value of repetition to appear in inverse proportion to the individual significance of the separate units; the more interesting or expressive the unit is in itself with individual significance, the less do we want it repeated; and so repetition of the human form must be conventionalized to the type (or to the same unvarying features), with enough individual differences still remaining to meet the demands both of the series and the individual. What apparent exceptions we have found to this rule have been shown to be meeting, in reality, another artistic demand.

Ends of Series and Arrangement of Repetitions within the Unit

The next question to consider is the *ends* necessary for a repeated series. Do they end with a heavier or with a lighter unit than the rest of the series, or with a unit of the same size? It will be remembered in the experiments touching this point that the subjects, without exception, preferred the series ending with heavier units. We

should then expect, in examples of repeated groups of posts, pillars, etc., alternating with wider or more prominent ones of the same kinds, that the series would end with the heavier or more prominent one. Examples of railings or balustrades alternating with heavier supports are so common, and the supports come so invariably on the end, that repeated examples seem almost unnecessary. But another question arose in connection with this: Does not the apperception of a group of lines equidistant from each other consist in going back and forth over them from edge to edge, with no rest on one point more than on another; while in a group of lines arranged at equal distances each side of the centre but not from each other, to emphasize bilateral symmetry, does not the attention rest on the centre, and move from the centre of one group to the next?

Moreover, we found that a wider space or embankment of some sort was necessary, to finish off a series of groups in which the separate lines were equidistant from each other, than to finish the groups whose lines were symmetrically arranged. This suggests that the activity which goes back and forth in the former case, being less coördinated and not bound to a middle point, needs more at the end to stop it than is needed in the latter case, when the attention is more upon the centre of each figure. It would seem, then, that the former arrangement would be appropriate for railings and balustrades, where the effect is of continuity either running wholly around the structure and into itself again or where a continuity of parts is desired and a connected series. The other arrangement divides the series into discrete parts. If the attention is stopped at every central point, the effect is less of continuity and more of separate unities bound together externally by their equal distances. We should, then, expect such series of units much less in continuous balustrades, but if they occurred at all, that they would be in connection with separate unities that did not want continuity or place in a series emphasized at the expense of their individuality. All this we might expect from the experiments alone, although whether such a refinement would have got into architecture seems questionable. Moreover, the question whether a symmetrical group of units needs a less heavy end to finish it than a group of the equidistant type is even more difficult to illustrate. Although the two types may be given under some conditions in experiments, in actual architecture they never appear so, for the two types never appear in the same buildings allowing them to be compared. Besides, few photographs are taken exactly in front, and no two at just the same angle. Any ac-

curate measurement of such end piers and any comparison of them is out of the question in the present methods of research.

One other question may be considered here. Does a series ever occur in which three units are repeated regularly, instead of one or two? In experiments we discovered that the subject found it impossible to feel repetitions of three in a series, and the only way that such a series was tolerable was when the three could be grouped somehow into one or two units. Therefore we should not expect to find such repetitions frequently, if at all.

To sum up: Do series always end with a heavier unit? Are units equally distant from each other more adapted to continuous or run-on railings, while units with symmetrical arrangements within themselves are found more often where separateness of objects enclosed is more aimed at than their connection? Is a less heavy end found after symmetrical series than after the other kind? Are repetitions of three units used at all, and if so in what way?

Obviously the only illustrations of these questions will be found in the arrangement of posts and pillars in balustrades of whatever description. In these cases alone do we find repeated series, with repetitions within the unit, as well as of the unit as a whole. The following examples have been taken by looking over about one thousand photographs and by recording every instance that occurred.

100 *Examples*

A. 73 Continuous Railings: Balustrades across façades; around roofs; up flights of stairs; around towers and baptisteries.

- I. 57 Rhythmic Units:
  - a. 31 Even numbers of units in group. Support arches 5.
  - b. 26 Odd number of units in group.
    - 11 Support even number of arches.
    - 6 More than eight units in group.
    - 1 Two sections of railing. Odd number in ends, decoration in centre section to emphasize symmetry.
    - 4 No grouping. Too many to count.
    - 4 Other reasons not assignable.
- II. 16 Symmetrical units: Slabs with carved reliefs or plain; Carved scroll or diamond designs alternating with posts; Heraldic designs on shields; Conventional decorations in stone or wrought iron.

B. 27 Detached enclosures: Separate windows and doors.

- I. 4 Symmetrical units:
- II. 23 Rhythmic unit-groups.
  - a. 10 Odd number of units in group.
  - b. 11 Even number of units in group.
    - 4 Support odd number of arches.

4 Although before separate windows make a continuous row across the side of the building.
2 Three sections of railing. Odd number in ends, even in centre section to emphasize symmetry.
1 No reason assignable.

c. Indefinite number in group. Iron bars in railings, and slender pillars on façade.

C. 8 Do not end on the heaviest unit.

D. No cases of regular repetition of three units.

Having 100 illustrations of repetitions of groups, with units repeated equidistantly between them, and of elements distinctly symmetrical, several new factors came to light. In all the one thousand photographs looked over, not a single instance was found of unit-groups with the units within, arranged at other than equal distances. There were many variations in the number of units in the groups; but the number being given, the units were arranged at equal distances from each other wherever the effect desired was of detached sections or of continued series. There are obvious structural reasons for this. Any repetition of groups for a balustrade or protective railing, which is the almost exclusive use of this variety of repetition, would be weakened by wider apertures on either side of the centre. A reasonably enclosed space is necessary to make the railing of value, therefore the specifically symmetrical unit as opposed to the rhythmic unit was found always in carvings, scrolls, bas-reliefs, etc., alternating with vertical supports. We should expect, then, in general, that in railings where an aspect of continuity of progress along some border or a tendency to go around an enclosure was sought, the units would be rhythmic in character, impelling one to motion and to carrying the eye and general organism out of repose into movement. We should expect, on the contrary, that symmetrical units would be found where repose or partial distinctness of the separate elements enclosed was desired, and where the attention was not to be carried away in so marked a degree. Seventy-three of the one hundred illustrations were of balustrades where the rhythmic factor was presumably aimed at.

The Rathaus at Braunschweig had a symmetrical design alternately occurring, but with four in a section, so that the section as a whole was not symmetrical and the attention was driven on, and in the other cases some other effect than rhythm was obviously aimed at. The genius of the structures was heavy and massive and the balustrade made in keeping with them, since an effect of motion or rhythm would have clashed with the spirit of the whole.

These examples have all been of the balustrades around enclosures, balconies, etc. Since the rhythmic unit has been found more fitting for them, we should expect, conversely, that in front of separate unities, such as windows, doors, etc., the symmetrical unit would be more in evidence. At first sight, the facts do not seem to bear us out in this. Of twenty-seven examples of separate windows, doors, and gates enclosed by railings, only four had distinctly symmetrical designs. (Casa Palladio, Bergamo Chapel, Petit Trianon.) These are wrought-iron designs in the centre with repeated rods on each side, or a row of six pillars with the central two larger and more decorated. Twenty-three, however, remain to be accounted for, and the solution of the difficulty is observed at once in the distinction between *odd and even* numbers. As was previously suggested there are obvious difficulties in having posts in a balustrade at any but equal distances, since the gaps left by unequal distances from the centre would destroy their reason for being. This difficulty can easily be overcome in wrought iron by extra central decoration, although it is not always done by any means; but in stone balustrades, unless there is carved open-work, or solid reliefs, there is no other choice than repeated posts, either divided into sections or continuous, and no variation is possible except to have an even or odd number of them. We should then expect that there would be an odd number in separate detached enclosures, bringing a post in the centre to emphasize the balance, while in a continuous series each group would have an even number, thus giving no centre to fixate upon, but driving the attention on without repose at any one point more than another. It might seem doubtful that any such refinement should have been actually expressed in architecture, but examination of these examples shows this treatment to be very general. Of the twenty-three examples of separate enclosed details, eleven have an odd number of posts. Of the ten that remain, *four* are examples of windows along the side of a building, with separate detachments of balustrade in front of each. By having an even number of group-units the continuity of the row is maintained in spite of a separation of the sections. *Two* of the ten are sections of balustrade over the central doorway of a building. These balustrades are divided into three sections, of which the centre is widest and the ends only half as wide. Thus, although there are six posts in the central section, the balustrade as a whole is distinctly divided into a bilateral symmetrical arrangement. *Three* of the others have an even number of pillars, but they support an odd number of arches;

and the arch, not the pillar, is taken as the unit of the repeated series. (Arches will be discussed later.) The *one* example unaccounted for represents a number of possible cases, where for some reason, following out a general scheme of building, or what not, the odd number is not insisted upon for separate clusters. But the fact that only one out of twenty-three is thus unexplained shows an unmistakeable tendency in the other direction.

A distinction between odd and even numbers cannot be felt above eight repetitions without actual counting, and often not even then.

The two final exceptions are of a gate and a decoration over a door (Fontainebleau, Piacenza) where there are nine or more units in the group. It is impossible to feel the system of this arrangement, and the result is proportionately confusing. A reservation must be made here concerning iron railings. There is no discrimination between odd and even in the number of iron rods in a section of railing and no tendency to symmetrical designs rather than rhythmic before detached enclosures. This is because from the nature of the case, there is no distinction possible between odd and even in the number of slender iron rods necessary to enclose a space with any security. There must of necessity be so many of them that the difference cannot be perceived, and so slight is the importance of each rod that the effect is more of a variegated surface than of actual beats of a rhythm. As soon as iron is wrought into large enough shapes, each repeated detail is of the same importance as in stone, but the slender rods commonly used in iron railings, although their repetition is rhythmic like all the others, give too slight a motor impulse to carry the attention past the heavy limits of whatever they enclose. They are found in front of many windows, but on account of the lightness of their rhythm compared with the solidity of limiting piers, no confusion results.

Having thus concluded that the odd numbers of units in groups is more adapted for separate enclosures, is the opposite true? In the continuous balustrade, previously discussed, are the units of groups made up of an even number of elements? Of the fifty-seven examples cited of continuous railings, thirty-one have an even number of posts in their groups. These conform to the rule: but what will explain the twenty-six remaining? It will be noticed that *six* of these have too many in a group for the eye to perceive any difference between odd and even, since they range from nine to thirteen. When so many units are in a group, the effect is always of the run-on type, whether the actual number turns out to be odd or even on

subsequent count. *One* has a balustrade with only two sections on a side, each side of the centre door. Seven are in each section, and since the appearance of a symmetrical whole is the desired effect, an odd number is more in keeping than an even; in fact, this example, Monte Berico, might better come under the other head of separate enclosures, although it partakes of the character of both. Another balustrade with three in a section (Blois Château) is so heavy and massive in all its parts that fixity and solidity is more in keeping with it than rhythm. *Eleven* of them, that is, the larger proportion of all those with an odd number of pillars in a section, support arches, and the arch is taken as the unit instead of the separate pillar; and we find an even number of arch-units in each section, which is what we should have expected. It is a noticeable fact, which was previously suggested in connection with separate enclosures, that when a row of pillars supports a plain lintel, the *pillar* is taken as the unit of repetition. (When the row is on the front of a building, temple, etc., the *opening* may be the unit, if the *purpose* of the central door or the fact of *going through* is in the mind: but when the series stands for itself, the *pillar* is the unit.) When pillars support arches, the *arch* is the unit, unless it is very narrow as in the Moorish style, when the pillar is often so high and the arch so narrow in comparison that its value is weakened.

Of the thirty-one balustrades with an even number of parts in a section, four sets of pillars bear arches, and make an odd number of them. This would seem to make an exception to the rule were they not so narrow in two cases that the pillar was still the unit, and in the other two the motif of the arch was built around the intervening piers, so that they did not seem divided into sections at all, but continuous.

We have thus surveyed the whole field of repetitions of rhythmic and symmetrical units, and their difference in treatment according to the end they serve, and the results bear out our expectations. The symmetrical unit, as exemplified chiefly by an odd number of units in groups, is more used for detached enclosures; and the rhythmic type, with even numbers, is used more especially for continuous ones. In the former case the motor tendency is toward the central balance, while in the latter it is driven on out of itself through the series. When pillars support arches, the arch is the unit; when they support lintels, the pillars themselves remain the unit. Any number of units over eight loses its value of odd or even, since the difference can no longer be perceived and becomes rhythmic whether odd or even.

It must not be supposed that these rules are inevitably carried out or that the effect is necessarily poor if they are not. It shows a general æsthetic demand, however, which in individual cases may be modified by other demands, or altered in parts to make a more unified whole. When, however, the series is taken for itself, and judged entirely on its own merits, these conclusions will be found generally valid.

We have still to consider whether series always end with a heavy unit. All the series examined *do* end in this way; in fact we feel the necessity of this so clearly that one illustration would be as good as a hundred. But there is a difference in the use of the end unit, which is noticeable in any two series of symmetrical and rhythmic units. Of the sixteen examples of continuous series whose units were distinctly symmetrical instead of rhythmic, eight of them, although ending on supports, do not end on the principal unit of the series. This can be best shown by one or two examples. The Orvieto Cathedral has on the façade a balustrade of rectangular reliefs alternating with supports. The reliefs are undoubtedly the more interesting and important element of the series, yet the series ends with the less important element, the support or post, and we feel that it must do so. The Palazzo Contarini has a balustrade on its façade in which carved wheel-like designs alternate with supports which come at the ends. Why, in these cases, do we feel it as inevitable that the heavier and more important unit should *not* come at the end, as with rhythmic units we feel that they should? The answer to this is partly structural and partly æsthetic. We must feel, first of all, that the series is properly supported, that it will not fall away at the ends or down in the middle, and for this reason support of some kind must come at the end to hold it up and give a feeling of solidity and stability. But why are not these supports made the more interesting and important unit so that they might still bear up the superstructure and end the series as well? Here the æsthetic demand appears. As soon as the object is regarded as an æsthetic unity and care put upon it to make it beautiful for its own sake, it must not be thought of as the *end* of any series. It must be cut off from the rest of the world by supports or framed in some way, and while it still may have a place in a series, provided it is sufficiently conventionalized and not too important in itself, it must not be thought of as either ending or beginning, as depending on a series to give it importance, or lending support to anything else. It simply exists, cut off from the world, even though in the balus-

trade not an integral part of it, and one ought to be able to remove it without affecting the stability of the structure.

The question whether series of symmetrical units have less heavy ends to finish them than series of rhythmic units cannot be settled by these methods of analysis. While it seems certain that the rhythmic series drives the attention on by its greater motor activity, and hence would need more of an end to stop it, so many other factors enter in of more importance, such exact measurements would be necessary (quite impossible with the photographs of the scale here used), the refinement would be so great, since the stone of which most of the examples are made, by its own weight supplies a check to rhythmic activity, all these considerations make it impossible to illustrate this conclusion and it must remain an experimental result alone.

There remains one question: Is regular repetition of three units ever found? They may be in combination of some kind so that they fall into a rhythm of twos, but are they ever found repeated as three separate and distinct units? The answer to this is without exception. Of the five thousand photographs analyzed, not one instance of this kind of series was found. In many cloisters the pillars are of different design, and often one design is repeated through an otherwise varying series, but their repetition is either without scheme of any kind, or in some combination that falls into a rhythm of twos. No three-rhythm has been used in art, any more than it has been found possible in experiments.

Arches

It has been noticed in the preceding discussion that when a series of pillars supports arches, the arch, not the pillar, is taken as the unit. If this is so, it would seem that the arch by binding two pillars together with a curve awakens a more vigorous response than the vertical line of the pillars, and this greater expenditure of activity makes it to be taken as the element of repetition. It suggested that the arch (like the rhythmic unit) tends to drive attention on out of one unit to the next in the series. The outward thrust of the arch arouses an outward-tending activity, and for this reason a row of *arches* would need, to give a finished, stable effect, a wider and heavier embankment at the end than a series of lintels. The experiments on this point were inconclusive owing to the difficulty of obtaining a series of arches and of lintels which should be comparable in size.

For this reason the validity of this suggestion must depend upon the actual treatment of arches in architecture. It would seem that the arch would, like the rhythmic unit, be more appropriate for continuous series than for detached short rows; or if the series were short, the ends should be treated in some way, by reduction in size, change in width of pillar, pier, or decoration, so that the outward-activity might be counteracted by some inward thrust or some accentuation of the centre. Thus the unity or balance of the series as a whole would prevent the arches from seeming to "run away" which they might appear to do without such treatment. We shall, then, look through photographs of buildings where arches are used, to find if their treatment carries out the supposition.

It may be seen at once that such a treatment of arches differs from the arrangement necessary to make plain lintels effective. The pillars on the front of Greek temples were indeed slightly farther apart at the middle entrance, and the centre was moreover further accented by the point of the pediment. But on the sides the rows of from thirteen to sixteen columns had equal interspace and no noticeably heavier columns or embankment of any kind at the ends, for none was necessary. The series appeared ended whenever it stopped, and did not carry the attention over, nor demand some finish to "hold it down," as does the arch. The pillars, to be sure, completely surrounded the temple, and so were, in name, continuous. But on a building with square corners, the other sides do not carry the series on to the eye (with variations in foreshortening of the ends) as in a circular structure, and the effect of continuity is not immediate.

Many examples might be given of buildings with pillars and lintels on the façade, which have no visible modifications of central or end columns to give balance or symmetry to the whole, and yet which are perfectly satisfactory as repeated series and do not demand either such treatment or further continuation, but are complete and finished: London, Trafalgar Square; Rome, Pantheon; Vienna, St. Karl, Barrome Kirche; Berlin, Schillerplatz, etc. These have the centre accented by the superstructure, but there is no discernible modification of the series itself.

Examples might be multiplied, but there are sufficient to illustrate the essential stability of repeated vertical units and to contract them with the outward-tending, run-on effect of arches which need various kinds of treatments to finish a series.

165 *Arch Series.*

A. 45 Go completely around exteriors: Colosseum, arenas, baptisteries, towers, cloisters, courts, basilicas, tombs.

59 *Series that end:*

B. I. 30 Central arch largest: triumphal arches, doors and windows on façades of churches.

II. 1 Central arch smallest: doors on Peterborough Cathedral.

III. 4 End arches larger: windows or decorative arches on the walls of buildings.

IV. 6 End arches smaller: windows, decorative arches, or arches halfway around a court.

V. 6 Arches go obliquely into higher central point and back: decorative arches running into the pointed roof on Romanesque façades.

VI. 6 Central arch accented by decoration: windows and gates.

VII. 6 End arches in different planes: doors on façades of buildings or in gates.

C. 20 Arches go around interiors: up naves and across the apse of churches, halls, and loggias.

D. 27 Around the outside of porches, apses, etc.; diminish in size at ends; are carried on in the transepts; motif is carried on, although whole arch is not; end arches are closed, or centres decorated.

7 *Good*

E. 14 Other arrangements: Roman aqueducts (endless); interlacing arches; filled with statues; finished by gables or turrets; bridges (land on each side a sufficient embankment); arches included in large ones.

7 ***Poor***

Series not sufficiently finished at the ends; only two arches in series; three arches, with first arch different from the others.

Of one hundred and sixty-five examples of such series examined, only seven do not conform to the principles we have considered, and these are proportionately unsatisfactory. Forty-five illustrate buildings where the arches go completely around the outside of a structure, so that the series instead of requiring an end simply runs into itself again. It will be noticed further, that unlike series of columns around rectangular Greek temples, these are around circular structures where the series does not change its direction suddenly but by degrees. With the exception of courts and cloisters where the observer stands within and sees the whole series, these are all around domes, baptisteries, etc., where the end arches in the field at any one point of view are seen in perspective gradually fading off and yet leading attention on around the building. There may indeed be arches which go across square-cornered buildings or even around them, but in these cases some other device is necessary to make each side

a finished series in itself. The mere fact of its continuance around a corner where it cannot be seen from the same point of view is not enough. (These various arrangements of arches on a flat façade will be taken up later.) Rows of arches are often used around towers square as well as round, but towers from their very shape and size allow the observer to see different sides from nearly the same point of view, so the series is not broken up into sections on different sides of the tower as it is in a larger building. Twenty more examples are of arches in interiors and are all of arches down a nave, with either a regular arch or an arch motif carried across the apse. It might be supposed that an arrangement of arches in an interior would be more difficult than on an exterior surface, since the genius of an arch is its outward thrust and its tendency to run on. Without careful treatment it would spoil the interior by trying to overstep its bounds; by making certain walls look wider than others; the arched sections utterly discrete in general character from the plain or otherwise decorated section. In point of fact, the use of the arch-series in interiors is quite conventionalized, and all the illustrations are of loggias, or of churches where the arch goes down the nave and in a more or less modified form across the apse. In the Sistine Chapel the arched windows go down the side walls and across the end in a vaulted double-arch. In some cases a series of Roman arches down the nave has a more or less pointed arch across the apse, but in every case the continuity has been kept in some way so that the series is unbroken. Moreover the columns in the cathedral naves are often so high and the arches so proportionally narrow that the pillar instead of the arch is taken as the unit. This is somewhat true in St. Mark, Venice, also in St. Sophia, Constantinople, where the large arches are divided into sections of seven smaller ones, each one of which is so narrow that the pillar is felt as the repeated unit instead of the arch; or if the arch be taken, the narrow span prevents it from too great outward thrust.

Thirty of the arch-series are on façades of buildings or in structures by themselves, as gates and triumphal arches, where the central arch is larger than the other, thereby emphasizing the middle point and drawing attention to it away from the ends. This centralizing a series or balancing it as a whole may be accomplished in various ways. Two examples make the central arch larger instead of smaller. Six make the end arches smaller while four make them larger. It will be readily seen that just which one of these variations is chosen for the series depends on the function of the series. The cen-

tral arch is wider, with only one exception, when the series is of arched doors and the central door is the main entrance; while the end arches are more apt to be varied when the series is purely decorative and serves no function. The central balance may be further gained by differences of level. In the decorations of many façades, especially the early Romanesque, rows of arches go obliquely into the point of the roof and by this strong pointing toward the centre create an inward tendency. Six of the illustrations have the central arch accented by decoration; seven have heavier piers around the central and end arches; six have the end arches brought out into a nearer plane which effectually finishes the series. All these examples illustrate the necessary disposition of arches on a flat wall or façade where the series in the field of vision must end suddenly, that is, cannot gradually fade away around a corner. The variety and yet invariability of these devices shows the need felt for some finish at the end, some balance of the whole with the central accent, which need, apparently, is not felt for pillars and lintels.

When the arch-series is on a circular structure, such as apses, porches, and the like, even when it does not entirely surround it, as an arena or spire, the regular diminishing of the series on either side, owing to the curve, supplies the finish necessary, and the size and arrangement of the arches need not vary otherwise. Twelve of the examples illustrate such a use of the arch, and although in some cases, Morano Cathedral, Nomantala Church, the arches are continued into the transepts gradually tapering in size, or are modified in size growing narrower from the centre, as in the Bergamo Church, such a treatment is not necessary for finished effect. The difference in proportion resulting from a curved series, or even on arches carried around a square corner (as in porches on Goslar and Braunschweig Rathäuser), where the series is open enough to clearly see its continuity as it runs into the main building, will suffice to make a series finished without modifications of the arch-units.

There are many instances of long rows of very narrow arches on cathedral façades which are too narrow to give outward tendency, or else they have statues within them which really take the attention and form a series of vertical units in place of the arches. There is also the common device of interlacing arches, where a supporting pillar of another arch stands in the centre of every arch, thereby always driving the attention backward and restraining it. Perhaps the natural outward tendency of the arch-series and the necessity for its limitation can be seen by violations of the principle. Seven

of the examples do not conform to any application of this rule and the results are not satisfactory so far as the mere series itself is concerned. Over the right and left doors of the Piacenza Cathedral are sections of nine arches which end abruptly and do not even meet each other. The Fredericksborg Schloss at Copenhagen has a row of fifteen arches enclosing a court. These run into wings on each side, to be sure, but all seen at once as they are and without central or end modification they are too sharply cut off and inclined to overstep their limits. The Loggia dei Lanzi at Florence, with its three wide arches and narrow pillars, the William Tell Chapel in Switzerland, with only two arches, illustrate forcibly the tendency of an arch to move outward, to appear too wide for the superstructure and too "active" unless bound down in some way. Four arches on the right and left of the façade of Marmonte Church, but not across the centre, have the same unfinished effect. The roman arch on one side of the St. Lo Cathedral façade with two gothic arches on the other defy every principle of repetition and symmetry as well.

From this survey of one hundred and sixty-five of arch-series we find through a variety of means a uniformity of purpose in their treatment; that all point to a common demand, however differently expressed, according to the function of the series. The series must be prevented from "running away." It must either run completely around a structure into itself, or be balanced as a whole so that the attention which naturally runs off the ends is driven towards the centre. This may be accomplished by enlarging, decreasing, decorating, or pointing toward centre of the arch by means of the obliquity of both halves of the series. It may also be brought by enlarging, decreasing, changing the plane of the end arches or altering the size of the limiting piers. The essential value of the arch may be altered by narrowing it, by filling it with something more important than itself, thereby making it only an attendant series upon its content, by interlacing it, or by any device that transforms or revises its outward tendency.

165 *Examples of Arch-Series.*

45 Go around outside a circular structure.
32 Go around interior and apses.
30 Central arch largest.
2 Central arch smallest.
4 Ends largest.
6 Ends smallest.
6 Central arch accented by decoration.
6 Central arch accented by upward incline of two halves.

6 Ends in different planes.
7 Different width of piers around centre and ends.
5 Very narrow arches.
9 Other reasons.
7 Unaccounted for.

The question discussed in the experiments, as to whether narrower interspacing was required between units decorated toward the centre, and units blank, or covered entirely with non-centrally accented decoration, could not be taken up in the latter analysis. To settle such a point, illustrations would have to be found of blank and decorated units of the same shape and size, in the same structure, and their relative interspacing compared. But no such examples were found, where the spacing was not regulated by some obvious structural reason other than pure pleasure in the repetition. This must stand, therefore, solely as an experimental result.

The use made of difference in plane or end, to facilitate two series being taken along together, whereas they would be fatiguing if the same in those respects, has been touched upon in the discussion of statues and bas-reliefs, and other series of more complicated units. Where the unit and alternate are both rich and significant, and would tire the observer by following each other at the distances they are obliged to be in a series, a slight difference in plane relieves the situation, and is used largely in monuments, fountains, pulpits, and such structures.

Many other questions have come up in the investigation which might be discussed in the same manner as the preceding, but can only be hinted at in conclusion:

Just what factors make an element and its alternate congruous? What is the exact relation of lines, which makes the scroll decoration in a balustrade alternate satisfactorily with an upright support, while the alternation of the arches in the Colosseum with the Greek pillars between them is incongruous?

In what does the pleasure in repeated series differ, when the observer is not certain just what is the repeated element? May there be a bare rhythmic pleasure, when the series is too far away to distinguish what the elements are, or when they run together, so that no definite demarcation is felt between them? Do such series excite a pleasure of repetition without content as to elements, and does it differ from mere variation and contrast?

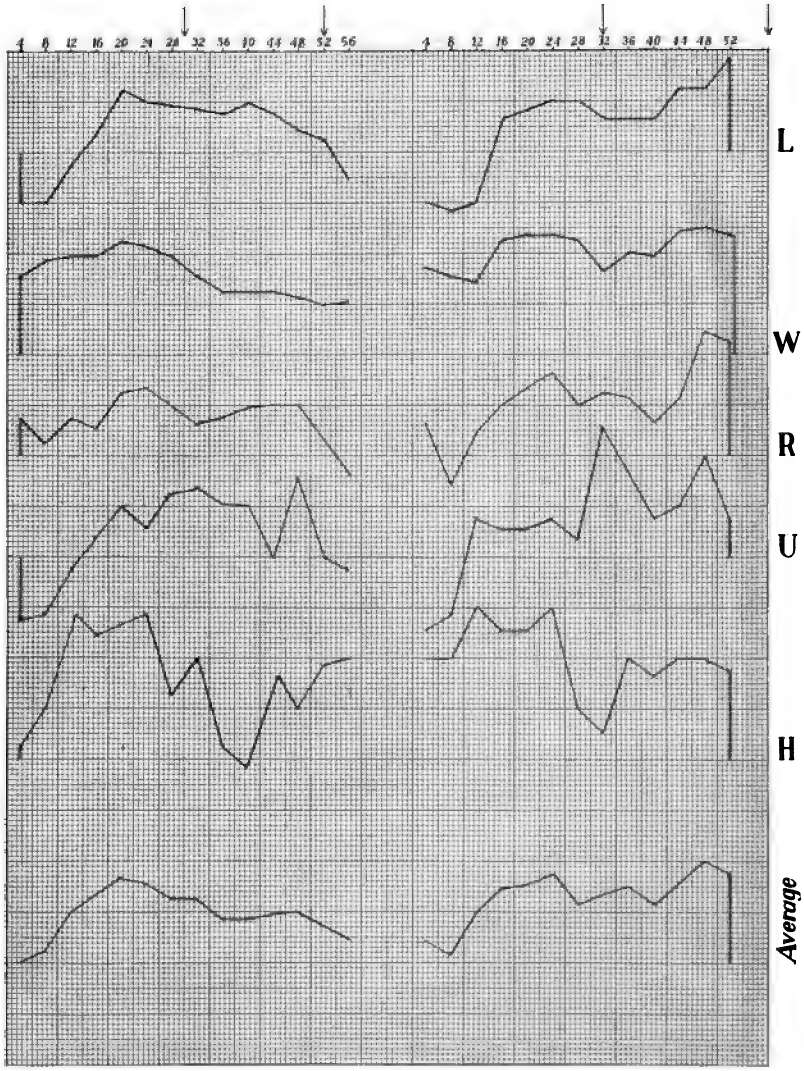
The series of unsymmetrical units was found in the experiments to have a peculiarly unstable run-on effect similar to that of rhythmic

units and of arches. Are they used in the same kind of cases as the others were, when a particularly active effect is desired?

Must a space be wholly enclosed, to be taken as a unit?

In a series of projections along a wall, the *projections* are taken as the unit, even when they almost meet at the top of the alternate space. When they actually do meet at the top, the enclosed space becomes the unit instead.

These questions and others similar might be experimented upon, and examples of their treatment analyzed, as in the previous questions discussed.



4 8 12 16 20 24 28 32 36 40 44 48 52 56
4 8 12 16 20 24 28 32 36 40 44 48 52
L
W
R
U
H
Average

PLATE V.

THE FEELING-VALUE OF UNMUSICAL TONE-INTERVALS

BY L. E. EMERSON

MODERN theories of melody start always with the presupposition that the scale must be composed of tones having the simple mathematical relation to one another of 2, 3, 4, 5, 6 (and by Meyer 7) and their multiples in order to give pleasant combinations of successive tones. But the question arises whether other tone-combinations which given together appear disharmonious may not, by their mere acoustical difference, similarity, and contrast, awake definite feelings of pleasure. And if such feeling-tones exist independently from harmony it is evident that they would enter into every melody in addition to the strictly musical feelings of harmony and that they deserve consideration as a factor of music. It would not even appear impossible that if every successive tone-distance has its particular natural feeling-character, the distances of successive harmonious tones might be only through secondary factors as habit and training preëminent among the various possibilities of combinations. A tone-consciousness, which under the guidance of experiences of harmony has been trained in our musical tone-relations, must give instinctive preference to such successions as our melodies offer. But if we artificially inhibit the conscious relation to our musical system by introducing a continuous tone-series, or at least one of steps much smaller than musical intervals, do we destroy the possibility of pleasure, and if not, do we find the pleasure in the musical interval stronger than that in other instances? That even the musical subject introduced into the realm of smallest tone-steps can easily forget and inhibit his normal standards is well known; the whole acoustical perspective seems changed by the new intervals, and the subject begins at once to build up a new temporary system of relations. The experiments in Wundt's laboratory have shown that in such cases the theoretical judgment of distances is indeed quite different from the standardized one; the octave may appear equal to the higher

fifth. I wanted to study in a similar way the feeling-value in such a state of musical disorientation, when all imaginative representations of our musical intervals are inhibited.

The instrument I used was an Appun Tonmesser giving reed-tones from 128 to 512 vibrations in intervals of 4 vibrations between adjacent tones. The intervals with which I experimented varied from 4 to 88 vibrations in steps of 4. The observers were all experimental psychologists, and varied in musical discrimination from a very low to a very high degree of natural ability and skill.

The observer reported his pleasure in the progression given, in the traditional grades of 1 to 7, where 1 represents the greatest degree of pleasure, 2 means very pleasant, 3 pleasant, 4 indifferent, 5 unpleasant, 6 very unpleasant, and 7 most unpleasant of all.

The immediate problem was: What is the relation between the width of interval used and the pleasure got by hearing the motive *a-b-a* and *b-a-b*, where *a* is always the lower tone. The method of procedure was to take a fixed tone (460 vibrations in the first case) and get a series of observations on successive progressions *b-a-b* where *a* differed from *b* by 4, 8, 12 . . . 56 vibrations. The greatest difference thus is approximately a musical whole tone. Then a series of observations was taken on *a-b-a* where *a* similarly differed from *b* by 4, 8, 12 . . . 52 vibrations. The progressions were given in irregular order, that there might be no chance of the observer getting into a fixed habit of replying. The intimate relation between the pleasure in successive musical tones and the pleasure in musical harmonies suggested naturally the question whether the feeling-value of these unmusical progressions was not somehow dependent upon the affective character of the simultaneous presentation of the same tones. Therefore after a progression had been given once and judgment recorded, the two tones used were given as a "harmony," that is simultaneously, and a judgment taken as to its agreeableness. This was immediately followed by the same progression, thus giving opportunity to observe the relation between the feeling-tone of the interval as it appeared in successive and in simultaneous presentation.

The results of this part of the investigation are graphically represented in the following plates. Tables I and II indicate the feeling-value of *a-b-a* where *a*, the lower tone, is 460 vibrations, and *b* is from 4 to 56 vibrations in addition, and the feeling-value of *b-a-b* where *b*, the higher tone, is 460 vibrations and *a* is from 4 to 56 vibrations less.

The base-lines from which the vertical lines to the curves are

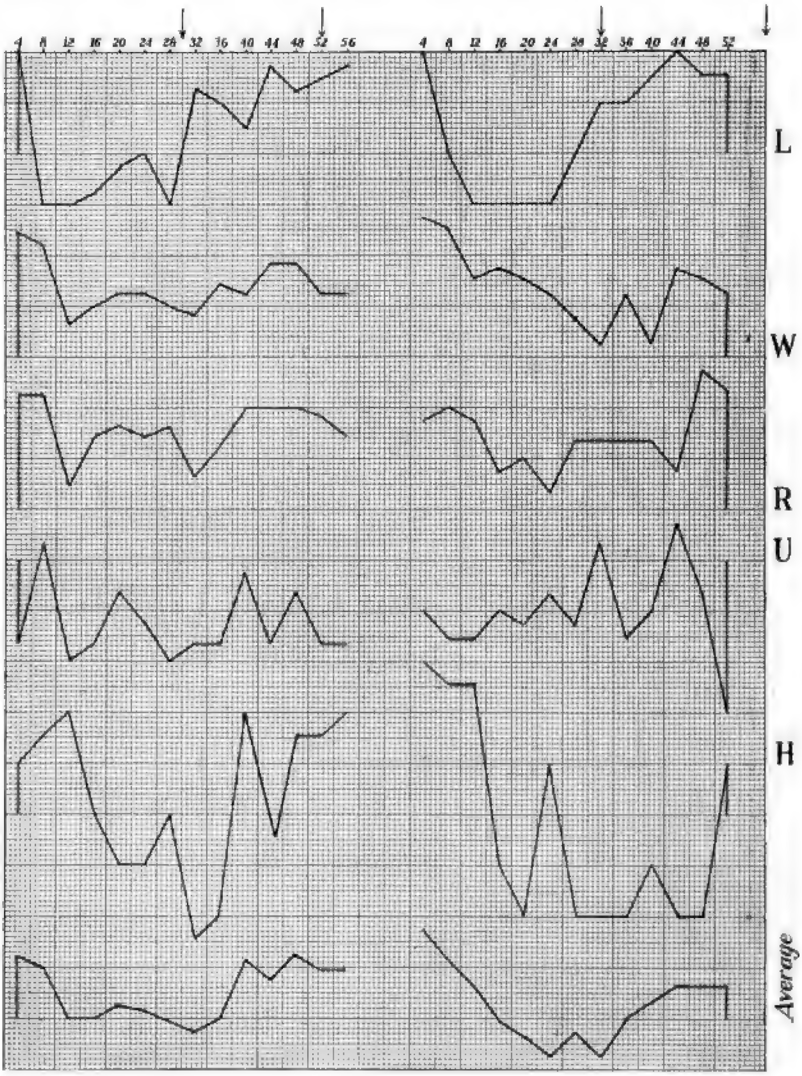

4 8 12 16 20 24 28 32 36 40 44 48 52 56
4 8 12 16 20 24 28 32 36 40 44 48 52
L
W
R
U
H
Average

Plate VI.

drawn represent the feeling-tone 4, the indifference-point. Above comes 3, 2, 1 and below 5, 6, 7; each square represents a unit. The horizontal abscissæ represent the width of the interval; the arrows indicate the musical intervals. The observers are given by initials. The first evident fact for both average curves of Plate V is that the maximum pleasure does not coincide with a musical interval, but comes with an interval four or eight vibrations less than either the half or the full tone of the musical scale. While in both cases the first elevation of the curve comes before the semi-tone, *b-a-b* shows a decrease of pleasure as the whole step is approached while *a-b-a* rises again. The order *a-b-a* is liked better than *b-a-b*.

Plate VI gives the "harmony" curve for the same tone-combinations, and it is clear at the first glance that the curves for the simultaneous tones do not correspond to those for the successive ones; in many respects they are directly the opposite. The hypothesis that the pleasure in such an amusical "melody" results from the resolution of the corresponding "harmony" is thus untenable; both are highly independent of each other. Yet, here too we notice the insignificance of the musical interval, while the strong pleasure in the tones different by 4 vibrations only refers probably to the complete fusion of the tones; there arises a direct enjoyment from the four waves of sound in every second, given by the beats. The pleasure-curve of these simultaneous tones indicates of course that the inhibition of the musical dispositions and expressions holds over from the successive to the simultaneous series. The pleasure is thus clearly different from that in real harmony.

Plate VII finally gives the "melody" curve for *aba* and *bab* with changes from four to four vibrations when the interval started with is larger than a full musical step. In *aba* the *a* is 384 vibrations and *b* varies from 436 to 516, the variations lying thus between the musical Second and the musical Fourth. It is evident that here again no feeling-preference is given to the musical intervals.

The question arises whether such small tone-intervals of amusical character allow the construction of more complex combinations of æsthetic value. Can we have amusical micromelodies with their own completeness and feeling of end? The following experiments represent a first step into this field. We used three tones only, *a*, *b*, *c* in 26 different combinations, and each of the 26 variations with intervals of 4, 8 and 12 vibrations between *a-b* and *b-c*. Each of the resulting 78 "melodies" was given repeatedly to six subjects in a time-order which allowed one second for each tone. The subject had to judge

on the pleasantness of the whole progression and had further to judge whether it produced a feeling of end or not.

The combinations followed in the experiments in this order: *abc, cbabc, abcb, cba, abcba, cbab, bcba, cbabcb, ababc, babc, abcbab, babcba, cbcba, bcbabc, abca, acba, acb, cbac, abcab, cabe, cbacb, acbab, cab, bca, cabcb, bac.* The lowest tone was varied between 200 and 444 vibrations; *b* and *c* were thus always still less distant than the next musical tone. The chief results may be shortly characterized as follows. There are hardly any judgments of indifference, the combinations are always decidedly pleasing or unpleasing. Of course a certain training in the apperception of such small-interval melodies preceded the real experiments and produced an attitude of adjustment to amusical relation. If we are in the midst of musical tone-relations and go over directly to such miniature intervals, we are seeking for the fulfilment of the habitual expectation and feel dissatisfied, or in the best case the procession is an indifferent chance combination. But as soon as a certain training with small intervals has inhibited the strictly musical expectations, a new setting of judgments with new standards comes in and a new source of pleasantness is opened. Of course even then no extreme feelings are to be expected; while the indifference-judgment 4 is lacking, the strong pleasure and displeasure, the judgments 1 and 7 are completely lacking too; three fourths of the judgments are 3 and 5. The pleasantness is decidedly more frequent than the unpleasantness, and this relation increases with the interval. The differences of four vibrations were especially with the higher tones hardly distinct for some of the subjects. Among 288 judgments in each group there were 150 pleasant and 138 unpleasant when the distances between *a-b* and *b-c* were four vibrations, 208 pleasant and 80 unpleasant when the distances were 8 vibrations, and 226 pleasant and 64 unpleasant when the distances were 12 vibrations.

The order of pleasantness expressed by the fraction of judgments of pleasantness and unpleasantness is the following: the largest number of pleasant feelings belonged to the figures *cbab* and *bac*, immediately followed by *abcb*; the further order downwards in affective value was: *cab, cbac, babc, abca, cbcba, ababc, abc, cabc, acba, cbabcb, bcba, acb, abcba, cba, cbabc, abcab, babcba, cbacb, acbab, bcbabc, abcbab,* and *cabcb* as least pleasant.

As to the feeling of end or æsthetic completeness the results are similar and yet independent. In a few cases the answer was "doubtful," but in the overwhelming majority a definite reply was given;

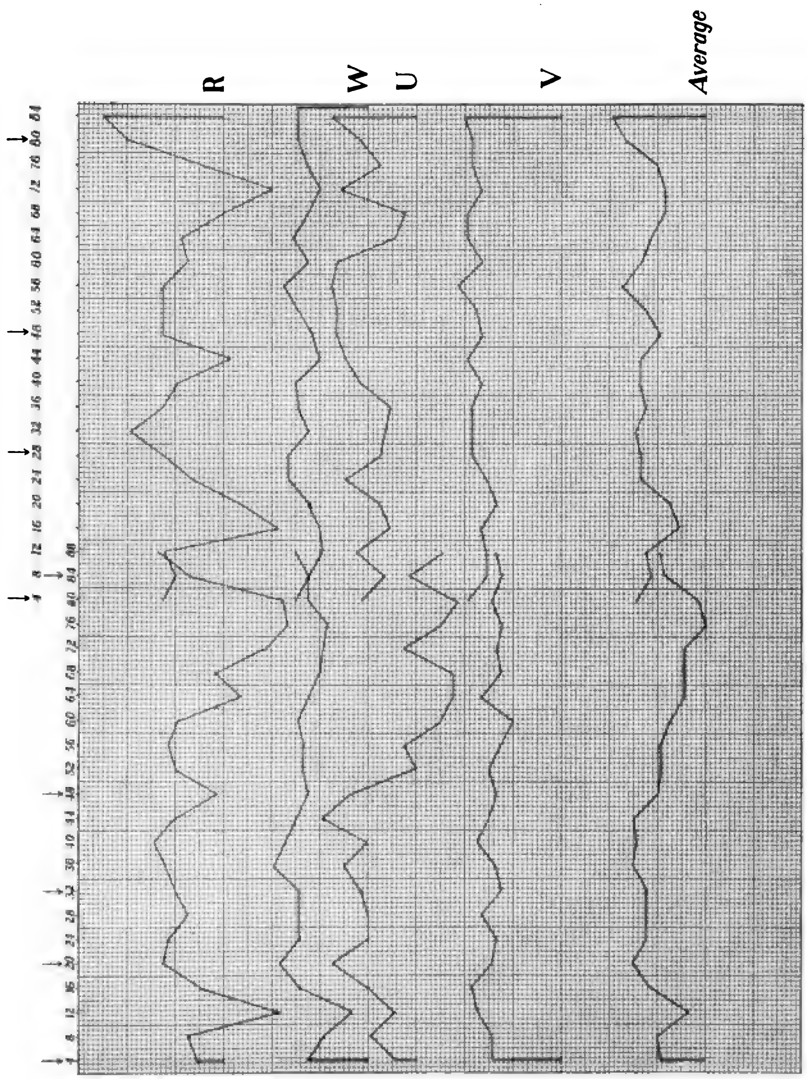


PLATE VII.

and while the judgment of completeness was by far more frequent in the pleasant combinations than in the unpleasant ones, yet often the unpleasant processions appeared as complete and the pleasant ones as incomplete. Here again the feeling of completeness grows with the interval, being smallest for the figures with distances of four vibrations. But most characteristic seems the fact that the feeling of end is in no way as in music dependent upon the return to the starting-point. The combinations which involved such return to the "tonica" show in no way a preponderance of judgments of completeness. If we order the results according to the number of this æsthetic factor the figures *acba*, *cbac*, and *cabc* stand very low, giving in the majority of cases the suggestion of not-completeness in spite of their return to the beginning, while the figures of the type *abcb*, *cbab*, or *cba*, or even the complex *babcba*, suggest in a majority of judgments the feeling of an end. The feeling of an end comes, according to the subjective reports of the observers, with an "internal unity of meaning" of the phrase given. This unity of meaning is here evidently quite independent from any simple mathematical relation.

The music-like quality of the figures was emphasized frequently in the subjective records. "I just enjoyed the progressions as music." "The elements are the same as in music." A melody of 384, 392, 400 was called a "very mournful strain"; 444, 452, 460 "Wagnerian motive; Tristan and Isolde"; and the same tones in another order "Very pleasant; expressed a pathetic resignation," or "Sounds like a little piece of music"; and so in most varied forms.

The basis of these experiments is of course by far too slender to build on them a theory, yet our results suggest at least a greater interest in the æsthetics of those tone-combinations which are excluded from our regular music. This interest is reënforced by the self-observations of all participants. They felt strongly that after all our musical pleasure in melody does not belong intrinsically to the tone-perception, but is learned and acquired like the grammar of our mother tongue. Such grammar too controls completely our internal demands for expression, and yet the learning of a different language can bring a new adjustment and a new set of psychophysical dispositions for linguistic demands. That whole apparently natural demand for the tone-combinations which give fusion and consonance can be inhibited during the listening to amusical combinations as soon as a short training in miniature intervals changes the acoustical perspective.

The development of instrumental music demanded evidently the

selection of distinctly separated tones and of intervals which give harmonious combinations. The external conditions of resonant chambers may have reënforced this selective process of historical music. It is certainly different with oriental nations, which produce music not in resounding chambers but in the free air and who are singers and not players, using instruments mostly for producing a mere body of tone as a background against which the melodies move; their intervals appear to our musical ear at first bizarre, and yet there too we are readjusted to the new dispositions for satisfaction with unsuspected quickness. We have no right to identify æsthetic pleasure in successive tones with the pleasure in our conventional music with the simple mathematical relations which alone give the pleasure of fusion; but being accustomed to this system of harmonies and being trained to expect it also in the resolved form of the melody, we need indeed an inhibition of habits and a certain new training till the more modest pleasure in amusical tone progressions comes to its natural right.

ASSOCIATION, APPERCEPTION
ATTENTION

CERTAINTY AND ATTENTION

BY FRANCES H. ROUSMANIERE

THE results of the experiments on the feeling of certainty which I have conducted fall into two divisions — those on the nature of the feeling itself, and those on the effect of voluntarily attending to certain aspects of a total experience upon certainty in the judgments as to the constitution of that experience. The problems of the first division are: Are there different kinds of certainty? In any one kind of certainty are there degrees, and if so, are these of a limited or an unlimited number? Can certainty be analyzed into elements? The problems of the second division are: Can it be said that in the report of any experience the judgments made with the highest degree of certainty will be confined to an attended-to group, and if not, will there be more there than elsewhere? In such a report will the direction of voluntary attention toward certain aspects materially alter the distribution of the judgments of the highest order of certainty over the various aspects of any given field?

These two divisions are so distinct in problem and result as to make it seem best to describe them as independent experiments. As some interesting results on the relation of error to the different grades of certainty and to the effect of attention developed in connection with this second division of the experiment, those results are given also.

In general the same subjects took part throughout the experiments. One, an instructor in Harvard University, whom I shall call K, was not subject for the second division of the experiment. Two others, E and H, both graduate students in Harvard University, could not serve as subjects in an important part of the first division. Of those remaining, B was a student in Radcliffe College, F an instructor in Harvard University, and A, C, and D graduate students in Harvard University. These last five were my subjects for all parts of the experiment.

I. THE NATURE OF THE FEELING OF CERTAINTY

The general method here was, of course, the method of introspection. Situations were created about which the subject might be expected to

make judgments with different sorts or different degrees of certainty, if such should be possible. He was then questioned as to his experience. The method has the fault of all introspective methods, viz., its results can in no case be verified. The results here are none the less suggestive, and, for the second problem, at any rate, definite enough to be convincing.

Most of the experiments were conducted in connection with visual fields. In working at the first problem which we have now to consider, however, the certainty connected with the dermal sensations and that connected with the simple reasoning process of addition were also examined. The apparatus used consisted of three sets of cards. On one set were pasted geometrical shapes cut from colored paper, and black and white letters or figures. Each of these cards was shown to a subject for a second and a half, or two seconds. After the exposure he told what he judged to be on the card, giving all that he could about the nature of his feeling of confidence (or certainty) for each judgment. On the second set of cards square pieces of tin, smooth rubber, rough rubber, cotton, felt, undressed kid, leather, eiderdown, flannel, coarse and fine sandpaper, and pricked paper were stuck, six on each card. The experimenter passed these cards so that these bits of material rubbed against the forefinger of the subject, while a curtain kept the card and the hand hidden from the subject's sight. Here, again, the subject judged of what had been on the card, just as he had done after seeing each of the first set of cards. Small sample cards, each having pasted upon it a piece of one of the substances used, were also behind the curtain, and the subject was allowed to feel of these as much as he wished while giving his report. Such sample cards were required because of the underdevelopment of the association of names of any kind with the dermal sensations. A single card with three groups of figures for addition upon it made up the third part of the apparatus. Here the subject was asked first to add the columns rapidly and to introspect as to his certainty of the correctness of the different results; then to go over the addition again, and yet a third time, and to compare his feelings of certainty in the different cases. The introspection was developed partly through the help of questions put by the experimenter, but in asking these questions great care was taken to prevent their influencing the judgment of the subject. Some observations made by the subjects during the second division of the experiment (also conducted in connection with visual fields) are, also, introduced here. Apart from this, the experiments on the feeling of certainty connected with this sense of sight were greater in number than the other experi-

ments; and it is those that have given us most of the data for answering the second and third problems.

The subjects did not agree in their answers to the first problem. Some found not only that the certainty connected with their belief in the results of their addition seemed to be of a distinct type from that connected immediately with the sense of sight, but also that there were different sorts of certainty connected immediately with the sense of sight itself. Others found but one kind of a feeling of certainty. All agreed, however, that so far as the kind or kinds of certainty associated with them was concerned there was no difference between the sense of sight and the dermal senses, so that it would seem to be true that any distinctions which are to be found within the feeling of certainty will not be distinctions springing from the difference in the sense-organs. Within the sense of sight, however, subjects B, E, and F divided their feelings of certainty into two classes, — an absolute feeling of certainty which they felt could not be shaken, and a feeling of confidence which they would act upon but which they felt might be shaken by questioning, and which seemed different by more than degree from the feeling of certainty proper. Subjects A, C, K and H found no such marked distinction between their feeling of greatest certainty and all lesser feelings of conviction. Subject D at one time felt that the distinction into two such distinct classes, the definitely certain and the more wavering, fitted his experience, and at another time said that it seemed to him that each degree of conviction stood for an unique feeling of certainty and that any two of them were as different from each other as any other two. A second division of the feelings of certainty into two classes is to be found with subjects A, F and H. This developed in connection with the visual experiments again. The distinction here may be called one into psychological and logical certainty. The latter rests on reasoning either from the probable character of the field, or from a feeling as to its general character, to the nature of some detail. We shall notice the characteristics of these two classes later. One subject, A, further distinguished as different the feelings of certainty connected with the two methods of logical certainty just given. The others made no such distinctions. In the experiment with the columns for addition only six subjects, A, B, C, D, F, and K took part. Of these the two who had made the distinction into psychological and logical certainty with the visual experiments (subjects A and F) again made the same distinction. Subject F, however, who had had occasion to do a good deal of important work with statistics, found practically no element of logical certainty in connection with his addition, though it

seemed to him that what confidence he felt in his result should be distinguished from the psychological certainty he had had as to the character of the visual fields. Subject B felt no certainty in her results except as she could so hold the process together as to have what seemed to her a simultaneous experience. When she had to judge of the results of a set of successive experiences that could not be so unified, she characterized her state of consciousness not as holding a feeling of certainty or of uncertainty, but as simply lacking any feeling of certainty. The other three subjects found no difference between the feelings of certainty and uncertainty associated with visual experiences and those associated with the process of addition. As a whole, it seems then that we must answer our first problem by saying that the case seems to be different with different individuals. With some the highest grade of certainty associated with a sense-experience is sharply distinct from the other grades, and with some again there appear at least the two general classes of psychological and logical certainty. On the other hand, there seem to be people for whom the feeling of certainty has no such sharp distinctions of kind within it.

The results as to the second problem may be more briefly and more distinctly given. No subject found any evidence that the number of the grades of certainty which he could distinguish would be limited by anything except his keenness in introspection, although in the simple tests given for the experiment, four was the greatest number of grades distinguished at any one time. Two of the subjects (B and F), who set the highest grade of certainty apart from the judgments made with lesser confidence, said that there might be degrees within that higher grade as well as among the "uncertainties." There was no evidence that logical certainty differed from psychological in respect of the grades to be found within it, and some evidence that they were alike in that respect, although logical certainty was less carefully examined. It would seem, then, that our second problem is to be answered thus: There are degrees present in some if not in all kinds of certainty, and there is no evidence that the number of these degrees is limited.

It was not generally found possible to analyze the feeling of certainty into a sum of elements, although certain characteristics seemed to be persistent in it. Here again there is marked individual variation. The general test used for the difference in degrees of confidence was the question "On which judgment would you risk more?" This satisfied every one as a true criterion for such distinctions, but subjects H and C said that for them the feeling of certainty had a much more distinct relation with the past than with the future. Perhaps for that reason,

subject H proposed the test "Which judgment could I be converted from most easily and simply?" The distinctness of an image had something to do with the feeling of certainty for subject C. Beyond this, he could not characterize his feeling. Neither was he sure that the degree of certainty varied exactly with the degree of distinctness. Subject D found that all objects about which he made judgments of which he was certain were present to his mind in the form of distinct images; but did not feel that that covered all that was to be said of the feeling of certainty. The number of images present, as visual and auditory, seemed to increase the degree of certainty for him. Subject F could give no characterization of his feeling of psychological certainty. His feeling of logical certainty seemed to spring largely from a feeling of consistency between the present experience and his past experiences. With subjects A, B, and K the vividness of an image was a strong determining factor in the degree of certainty felt in any judgment, but again was not the whole story. Something they could not characterize was also present for A and B, and, as well, a feeling of more or less perfect congruence between an image and the general character of a field. (This introspection developed in connection with the visual experiments.) Among these eight subjects we have but one (K) who is satisfied with reducing certainty to a set of elements.

To my mind the most valuable thing to be gained from this division of the experiment is the suggestion that there are definite types of certainty, and that people may be classified by these. There are obviously marked individual variations as to the characteristics of this feeling. I should expect from my work this year that two pretty distinct types could be discovered. For one of these, certainty in a judgment as to an experience would rest very largely upon the vividness of an image; for the other, upon the congruence of an image with other previously accepted images, that is, the absence of conflicting images when the experience judged about is imagined part of a wide setting of past experiences. I should not expect either element of certainty to appear absolutely, without the other form. For many people one element would predominate in certain fields, as in judgments regarding sense-experiences, the other in the more logical fields. For some, again, perhaps, the two would be nearly coördinate in every experience of certainty. But for some subjects, as, I think, for subject K here, the vividness of the image would always be the determining factor, while for others, as for subject H, congruence with wider experience would be much more important. This classification of subjects according to their types of certainty might develop into a much more complicated affair. The

experiments described here have gone no farther than to suggest lines along which it may perhaps run. There may be other elements equally important with these two. A set of experiments consisting of attempts to raise uncertainty to certainty would bring out the essentials of certainty from a new point of view, and would, perhaps, test this theory that individuals may be classified according to the types of their certainty, in the most satisfactory manner.

II. The Effect of Voluntarily Attending to Certain Aspects of a Total Experience upon Certainty in the Judgments as to the Constitution of that Total Experience.

As has been said, judgments as to the elements of visual fields were tested for this part of the experiment. The apparatus used was the following: The subject was seated before a low table which was shut from his view by curtains and boards. He looked down upon the table through an opening into which a camera-shutter had been fitted. This shutter was set for a two seconds' exposure and opened by means of a bulb which the subject held in his hand. Just before each exposure, the experimenter placed a card on the table below the camera-shutter. The set of twenty cards so used were alike in that the background for all was gray and the objects pasted upon the cards black letters and numerals and simple geometrical figures of chosen shapes and colors. No color was repeated on any one card. The cards were different in the choice and arrangement and in the number of objects used. The number of letters and numerals on any one card varied from two to five, the total number of objects from eight to twelve. A white card on which were pasted dark gray samples of each of the eleven shapes used, together with a card of the background of those shown in the experiment on which were pasted torn scraps of the eleven colored papers used, was always in sight at the subject's side. A camera-shutter, experiment cards and sample cards thus made up the apparatus.

The presence of the sample cards needs explanation. They stood for the attempt to place the colors and shapes on the same footing as the letters and numerals. Their presence, in the first place, and, as well, the limitation of the number of letters and numerals used, did away somewhat with the advantage that letters and numerals naturally have for ease of naming. In the second place, the use of a new color for the sample shapes and the absence of definite shape in the sample

colors helped to keep the colors and shapes more distinct. With the help of these cards it seemed that we could properly hold we had a a visual field of three very nearly coördinate sets of elements.

The experiment as a whole, as conducted, had four phases which, except for one particular, were exactly alike. The subject's attention was directed toward a certain aspect of the field by (1) asking him before each exposure (or less often if that appeared unnecessary) to attend to that aspect, as, for instance, to the colors present, and (2) taking care that any questions asked should tend to strengthen rather than counteract the effect of that voluntary attention. At a given signal the subject pressed the bulb which opened the shutter. On the closing of the shutter he reported what he had seen. This report the experimenter recorded almost in the subject's own words, and later tabulated in the manner described presently. So far as giving the objects present was concerned, the report was given almost invariably without any suggestion by the experimenter as to the possibilities of the field. To help the subject distinguish the amount of confidence which he had in the judgments that such or such objects were present, however, the experimenter frequently asked such questions as, "Would you risk more on the fact that there was a square in the field than on the fact there was something blue there?" In giving his report the subject pointed to the sample cards or spoke, as he might wish. He was also allowed to be as leisurely or as rapid in giving it as he chose. A half-minute interval elapsed between the end of each report and the signal that the shutter be opened again. No persistent effort to distract the subject's attention was made then, though conversation on other topics was frequently carried on. The point in which the phases of the experiment differed was in the aspect of the field to which attention was called. In the first, this was the shapes, in the second, the colors, in the third, the letters and numerals, and in the fourth, the number of objects in the field. Fixing the attention upon the number of objects in the field served to distribute it equally over all the groups represented there. The general method of calling attention to the different aspects and of learning the effect of such attention was, as has just been said, the same for all phases.

As a preliminary to making up the tables here given, from which we are to answer our problems, the experimenter first tabulated the reports of the subjects in such a way as to show how many judgments (correct and incorrect) of each of the four grades of certainty adopted for this division of the experiment were made by each subject on each card for each group on the card (shape, color, or letter or numeral).

From these tabulations the tables that follow were in turn compiled.

The number of grades of certainty adopted for this division of the experiment is obviously decidedly arbitrary. Grades of certainty there surely are. The introspection of the subjects develops that clearly, as has been stated. But there is no reason in the conditions of the case for holding to the number four, as is done here. In giving the results for which the experiment was undertaken, I shall, indeed, confine myself to studying the range of the judgments made with as high a grade of confidence as the subject believed he should ever have. This is called certainty (1) or certainty proper. But for the tributary discussion on the relation of certainty and error, the consideration of three other grades used in the report and early tabulation, is also introduced. This lowest grade (4) might better be named "as complete uncertainty as will admit of one's making any judgment." The other two are intermediate. It was at first intended to give the results with regard to the effect of voluntary attention upon the place of these grades of certainty, also, but such a discussion has been omitted because it promised to add very little more than complexity to the report. Besides this, the classification into these lower grades is too purely approximate to make the distinction there of great value. For judgments of the order certainty (1) we have the test, "Are you as certain of this as you can imagine being in an experiment of this sort?" but no such test for the other grades could be found. Yet, though he tended to omit judgments of the lower grades of certainty, each subject seemed to find four grades a convenient number to use in giving his report.

The number of experiment cards used varied with the subjects. E had so clear a memory of the cards that after as many as ten had been shown, he found difficulty in distinguishing his memory of the one which he had just seen from that of others seen earlier. Ten cards only were used in his case. A, B, D, and F showed signs of fatigue after fifteen cards which made the value of any later results questionable. C and K showed no such signs of fatigue. The same set of cards was, of course, shown any one subject for all four phases of the experiment. Those omitted were the last ten or the last five of the complete set as the case might be.

TABLE I

	% of cards where certainty (1) appears in the attended-to group.	% of cards where certainty (1) appears elsewhere than in the attended-to group.	% of cards where certainty (1) appears in attended-to group only.	% of cards where certainty (1) is stronger outside than within the attended-to group.
A	91%	49%	49%	13.3%
B	97	91	83	28
C	95	67	30	16.6
D	93.3	69	24.5	11.2
E	96.6	83.3	13.3	26.6
F	88.8	30	53.3	8.3
H	91.6	70	26.6	10

Table I answers the first part of our first problem promptly. Every subject gave judgments of the order certainty (1) about groups other than that attended to, in the case of a very considerable percentage of the cards. True, again in the case of a considerable (though generally smaller) percentage of those cards, each subject confined his judgments to the group attended to. The fact of individual variation stands out again here; and, moreover, the conclusions drawn should be qualified slightly because of the fact that it was often possible for the subjects to give all the letters and numerals on the cards, and still have, as it were, some attention left over for the other, supposedly non-attended-to groups. Such reaching beyond the properly attended-to group never seemed to be possible with either shapes or colors. Aside from this, however, it is clear that judgments of the highest grade of certainty were by no means limited to the group attended to.

This same table answers, also, the second part of the problem. Each subject found certainty of the highest grade sometimes stronger outside than within the group which held his attention. It is, of course, practically impossible to make absolutely certain that each subject's attention was invariably held to the group toward which it was turned, yet the percentage where certainty was stronger outside than within such groups seems large enough, in some cases, at least, as with subjects A, B, and H, to warrant our answering this second part of the problem in the negative. I should feel, however, that this was answered less definitely than was the first part of the problem. We may say, then, that the judgments made with the highest degree of certainty about a visual field will not be confined to the group attended to, and that we have strong evidence pointing toward the belief that we cannot expect there will invariably be more of such judgments within the group attended to than outside it.

TABLE II

			% of judgments of certainty (1) given to each group in phase I (or when shapes were attended to).	% of judgments of certainty (1) given to each group in phase II (or when colors were attended to).	% of judgments of certainty (1) given to each group in phase III (or when letters and numerals were attended to).	% of judgments of certainty (1) given to each group in phase IV (or when the attention was equally distributed over all the groups).
Subject A	Shapes	(a)	94%	38%	18%	31%
	Colors	(b)	5	61	20	59
	Letters and Numerals	(c)	0	0	61	9
Subject B		(a)	48	39	21	33
		(b)	43	60	29	38
		(c)	8	0	51	28
Subject C		(a)	88	15	26	34
		(b)	8	77	28	8
		(c)	5	7	47	58
Subject D		(a)	60	13	11	40
		(b)	19	67	2	37
		(c)	19	19	86	23
Subject E		(a)	51	15	0	37
		(b)	22	56	8	29
		(c)	26	28	91	34
Subject F		(a)	66	12	0	50
		(b)	14	77	0	27
		(c)	19	10	100	23
Subject H		(a)	43	21	7	24
		(b)	29	52	4	19
		(c)	27	26	88	57

The most interesting part of this division of the experiment is brought out in Table II in answer to the problem, "Will the place of voluntary attention materially alter the distribution of judgments of the highest order of certainty among the given groups?" In every case the percentage is affected, in most cases, greatly affected. Take the case of subject A, for instance. Although, when his attention is equally

distributed over the field 59 % of the judgments we consider were of colors, yet when his attention was fixed on shapes and on letters and numerals this fell to 5 % and 20 % respectively. When it was fixed on colors, it rose, indeed, only to 61 %. When, however, subject A fixed his attention upon the letters and numerals, 61 % of the judgments were confined to the group attended to, — the same percentage as when colors were the attended-to group, — although, when his attention was distributed over the whole field, the percentage of these judgments about the group of letters and numerals was 9 % only. When shapes were attended to, the 31 % of the fourth phase of the experiment rose to 94 %, — almost all of the judgments of the highest grade of certainty that were given were judgments about shapes. A similar study of the results given in the table can be made for the other subjects. The degree of change varies with the subject and with the group, but always there is some change, and often a very marked one. In this experiment the place of voluntary attention clearly did alter, and alter materially, the proportion of judgments of the highest order of certainty made about any given group.

That, indeed, would seem to me to be the answer of this experiment to the question as to the effect of voluntary attention upon certainty in one's judgments. Every subject showed a tendency to have more certainty in those judgments which were made about that aspect of the field toward which his attention was directed. Yet, on the other hand, this was a tendency only, one not strong enough to make it possible to predict beforehand exactly how great a proportion of the judgments in which he had the highest degree of confidence would be limited to that field, or even to be sure in every case that the greater proportion of those judgments would be so limited. The place of voluntary attention has an influence upon the subject-matter of the judgments made with certainty about a visual field just seen, but an influence of varying and uncertain strength.

TABLE III

x = no judgments of that kind given.			% of mistakes in judgments of certainty (1).	% of mistakes in judgments of certainty (2).	% of mistakes in judgments of certainty (3).	% of mistakes in judgments of certainty (4).
Subject	(in giving shapes)	(a)	7%	10%	0%	100%
A	(in giving colors)	(b)	2	14	23	0
	(in giving letters and numerals)	(c)	0	0	x	x
Subject		(a)	2	10	10	0
B		(b)	3	6	20	25
		(c)	4	50	0	x
Subject		(a)	1	8	10	0
C		(b)	4	14	14	0
		(c)	1	0	16	0
Subject		(a)	3	4	0	16
D		(b)	1	6	0	0
		(c)	5	0	0	0
Subject		(a)	22	10	25	50
E		(b)	1	33	40	0
		(c)	0	0	0	0
Subject		(a)	1	25	7	25
F		(b)	4	15	29	26
		(c)	3	0	0	x
Subject		(a)	3	6	5	0
H		(b)	6	2	15	15
		(c)	4	0	0	20

TABLE IV

	General % of mistakes in judgments of certainty (1).	% of mistakes in judgments of certainty (1) about attended-to groups.	General % of mistakes in judgments not of certainty (1).	% of mistakes in judgments not of certainty (1) about attended-to groups.
Subject A	4%	1%	17%	27%
Subject B	3	2	10	7
Subject C	2	3	9	24
Subject D	3	4	4	0
Subject E	1	2	22	34
Subject F	3	1	21	23
Subject H	4	6	6	10

The results given in Tables III and IV were compiled from the same records as those of the two Tables just discussed. They give the relation of error to certainty and to attention, as that relation was developed in this experiment. No experiments were conducted with these relations of error primarily in view, but the results developed in connection with the problem of the effect of attention upon certainty in one's judgments.

Both Tables show again marked individual variation. They suggest to me, in the first place, a further line of investigation in the same field and for the same purpose as those investigations which L. William Stern outlines in an article[1] entitled *Aussagestudium.* This further line is the testing subjects to learn the probable relative correctness of the judgments made with different degrees of confidence. Although a comparison of the first and third columns in Table IV makes it clear that the proportion of mistakes for the highest grade of confidence is lower than for the other grades taken together, there is a very marked difference among the subjects to be noticed. The difference in the two percentages is, for instance, very slight in the cases of D and H, and very great in the case of E. It is interesting to notice with regard to E that while he has the lowest percentage of mistakes for certainty (1), he has the highest percentage for the group of cer-

[1] Stern: Beiträge zur Psychologie der Aussage, vol. 1, p. 46, 1904.

tainties (2), (3), and (4). In the more detailed percentages given in Table III we see further that in certain fields and sometimes in all fields (as with subject C) judgments made with the lowest grade of confidence were invariably correct. Such Tables might be of help in a case where the evidence of eye-witnesses conflicted. We might perhaps learn that witness N made a large proportion of mistakes where he was absolutely certain, whereas witness M was seldom wrong in judgments in which he had a low degree of confidence. Even when the probity of both was unquestioned, we should not then assume that N was more probably right because he had so much more confidence in his judgments than M had in his. A much longer and more comprehensive set of experiments would be necessary before we could feel that we had at hand a table from which to work in this way.

The question of the effect of voluntary attention upon error, for answering which Table IV was compiled, brings out again the marked individual variation among these seven subjects which has shown itself in practically all parts of the experiment. Some effect seems to have been produced always, but this was sometimes to give a larger percentage of mistakes in the attended-to groups and sometimes a smaller. With A, B, and F the percentage of mistakes in certainty (1) was lower for the groups attended to than for the total number of judgments of that order. Only with subject B, however, is this true of the group of lower grades of certainties also. On the other hand, with subjects C, D, E, and H the percentage is greater for certainty (1) in the groups attended to than for certainty (1) in the collection of all the judgments of certainty (1) taken together. Here, too, in the case of subject D, the results with regard to the lower grades of certainty reverse those for certainty (1). Thus all four possibilities as to the kind of influence of voluntary attention upon certainty appear. We cannot say that the place of voluntary attention will tend to affect the percentage of error in any given way. We can only say that apparently it made some difference with each subject. It might be found by further experimenting that the character of this difference is associated with some other characteristic of either attention or the feeling of certainty, as, for instance, with the ease with which attention is held to the chosen field or with the type of the subject's certainty.

Like all experiments, these open up further questions quite as much as they answer those toward which they are aimed. To repeat something of what has already been said, I feel that what it has established is (1) that introspection develops distinct grades of certainty in the

case of every individual, (2) that the particular characteristics of the feeling of certainty vary markedly among individuals; (3) that the feelings of certainty associated with the different senses are not, as feelings of certainty, to be distinguished from each other; (4) that the judgments of the highest degree of certainty which are made about the constitution of any visual field just seen will not be confined to the group in that field toward which the attention is directed; and (5) that such fixing of the attention will, nevertheless, materially alter the subject-matter of such judgments of greatest certainty. The rather vague statement that the percentage of error is not surely less with the judgments of a group because attention is fixed on that group may perhaps be added as a sixth conclusion. The most interesting and promising of the problems which the experiments seem to me to raise are: (1) the problem, are there such definite types of the feeling of certainty that people may be classified according to their types, and, if so, what are the types and what their relation to other psychological characteristics of the individual? (2) the problem, what will be the result of careful and trained introspection as to the relation of so-called logical and psychological certainty and in what fields do these appear for different individuals? (3) the problem, how can a test for grading the probable percentage of error in the judgments of different grades of certainty made by any one person be constructed? and (4) the problem, how are such facts as those given in Table IV to be connected with the effort required for attention, the type of certainty of each subject, etc.? Other problems could, of course, be suggested, but these, I feel, mark the steps that naturally follow the experiments described here.

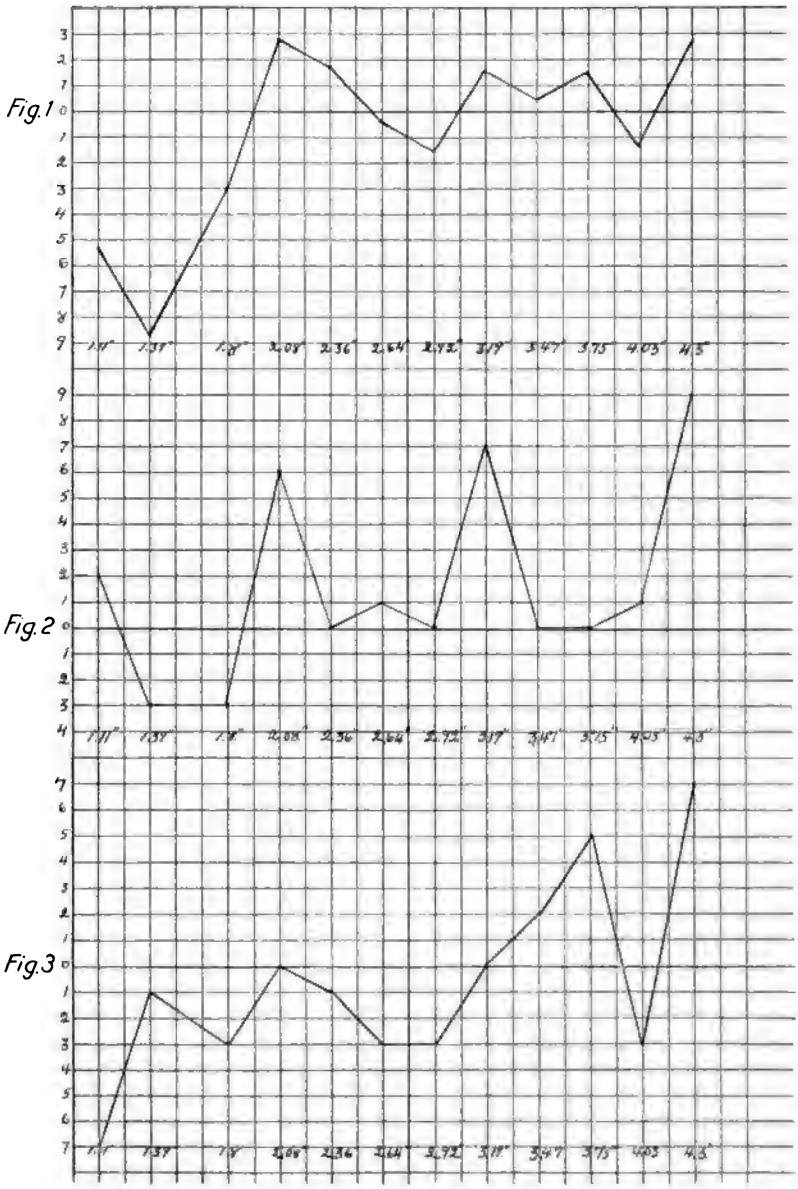


Plate VIII.

INHIBITION AND REËNFORCEMENT

BY LOUIS A. TURLEY

EXPERIMENTS made by Ranschburg[1] on the significance of similars in the process of learning and remembering determined that when duplicates occur within a series of stimuli, one either totally or very greatly inhibits the perception of the other according as they are contiguous or are separated by other stimuli. Dr. Yerkes,[2] in testing the effect of auditory on visual and tactual stimuli in frogs, found that if the auditory stimulus preceded another stimulus by various time-intervals, it had an alternating reënforcing and inhibitory effect. A similar result was obtained by Hofbauer[3] in a similar experiment on human subjects. The question now arises, — if the time-interval were increased between a stimulus and its duplicate in a series would the inhibitory effect gradually approach zero where all effect of the preceding stimulus ceased, to which Ranschburg's experiments point, or would the inhibitory effect be alternated with one of reënforcement as the experiments of Dr. Yerkes and Hofbauer would indicate? This problem — the effect of a stimulus on its duplicate in a succeeding series of stimuli — is the problem I undertook to solve. For this purpose, it was necessary to introduce exactly determinable time-intervals between the stimulus and its duplicate. Therefore I used — as Miss Kleinknecht[4] did for other purposes — a stroboscopic arrangement instead of simultaneous presentation which Ranschburg used.

My apparatus was Professor Münsterberg's Stereoscope without Prisms or Lenses, a description and photograph of which was pub-

[1] Ranschburg: Ueber die Bedeutung der Ähnlichkeit beim Erlernen, Behalten und bei der Reproduction, Journal der Psychologie und Neurologie, Bd. 5, p. 94.

[2] Dr. R. M. Yerkes: The Sense of Hearing in Frogs, Journal of Comparative Neurology and Psychology, vol. 15, no. 4, 1905; also this vol. Harvard Psychological Studies.

[3] L. Hofbauer: Interferenz zwischen verschiedenen Impulsen im Centralnervensystem, Pflügers Archives, Bd. 68, p. 564, 1897.

[4] Kleinknecht: This volume.

lished in the article by that title in Psychological Review, vol. 1; or rather, I used Professor Münsterberg's attachment to Kohl's centrifugal machine, since my apparatus was not identical, except in principle, with the "Stereoscope." The "attachment" consists of two black discs about thirty inches in diameter, mounted about eight inches apart on the disc-shaft of the centrifugal machine. The back disc is of wood. The outer three inches of its face is furnished with thirty-six equidistant strips of black tin, one end of each of which is bent so as to grip a groove in the rim of the disc, and the other end of each is gripped by tiny thumb-screws so that the strips lie along radii of the face of the disc. The front disc, slightly smaller than the back disc, is of pasteboard. Between the two discs a stationary black screen with a short narrow slit was placed so that the slit revealed only the strip on the horizontal radius of the back disc. Behind this screen an eight-candle-power electric light was placed to illuminate the back disc,—as the experiment was carried on in a darkened room. By moving this light I was enabled to vary the intensity of illumination to offset the skill of the observer.

For my purpose, a small white figure — one of the ten characters of the Arabic notation — was stuck on about the middle of each of the tin strips on the back disc; and radial slits, one millimetre wide and an inch long, were cut from one sixth of the circumference of the front disc so as to come opposite six of the strips on the back disc. Similar radial slits were cut at various intervals from the remaining five sixths of the circumference of the front disc. These were covered by small pieces of cardboard fastened to niagara clips, thus making them readily removeable. By this means any desired figure could be exposed in the same revolution with the series exposed by the six slits above mentioned.

The thirty-six strips were divided into six series of six each, indicated by chalk-marks on the disc. Each of the series was often changed in whole or in part by shifting and interchanging the strips.

The figure on which the effect of a preceding stimulus was tested occupied the fourth place in the series, since this is the place where the greatest number of errors occur, as is shown by the experiments of Ranschburg and previous investigators in the Harvard Laboratory. In my experiment, 4, 5, 6, 7, 8, and 9 occupied the fourth place in the 1st, 2d, 3d, 4th, 5th, and 6th series respectively, and the effect of a preceding stimulus was tried on each of these figures for each time-interval. The preceding stimulus in each case was a duplicate of the fourth member of a series, and was a member of some

Fig. 4

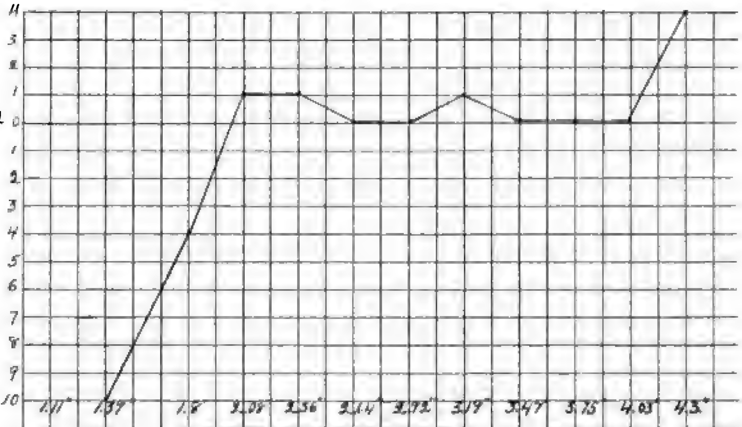


Fig 5

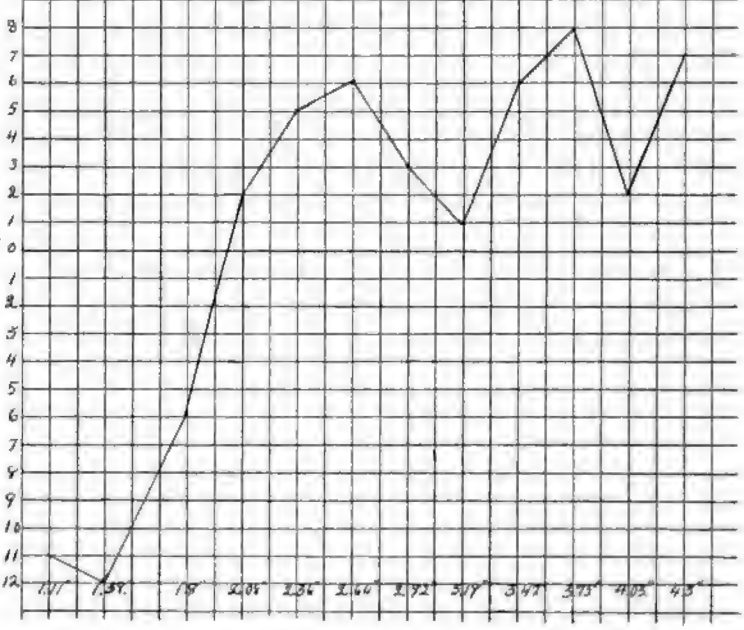


PLATE IX.

other series. Thus the fourth member of each series was at all times fixed and constant while the preceding stimulus occupied successive progressive positions round the disc. The other members of each series were chosen at random, care being taken that the fourth figure was not duplicated within its series, since it would then have taken part in inhibition within the series.

By adjusting the inner disc, I exposed any one of the series desired, and by removing the cardboard blind from one of the suggestion slits, I gave a stimulus at the desired time-interval in advance of the fourth member of the series. The first interval I used was 1.11 sec. as Miss Kleinknecht had tried intervals up to 1 sec. My second interval was 1.39 sec., the third 1.8 sec., and then every .277 sec. up to 4.3 sec. In performing the experiment I exposed alternately a series without and a series with a preceding stimulus—taking from the observer three reports of each—until the six series had been seen. I then repeated this, exposing with a preceding stimulus those series that had been exposed without preceding stimulus, and without preceding stimulus those series that had been exposed with a preceding stimulus in the first instance. In this way I equalized and minimized the effects of novelty and memory.

At 1.11 sec. there was considerable inhibition in five out of six cases. In the sixth case there was slight reënforcement at this interval. With an interval of 1.39 sec., with one exception,—not the exception above mentioned,—there was a stronger inhibition than at 1.11 sec. Inhibition in all cases began to decrease from 1.39 sec. until it ceased at about 1.8 sec. The preceding stimulus then had a reënforcing effect which reached a maximum in four cases at 2.08 sec., one at 2.36 sec., and one at 2.64 sec. Then, in all cases, there was a decrease of the reënforcing effect which in three cases amounted to inhibition. In the other three cases, the preceding stimulus had no inhibitory effect for an interval greater than 1.8 sec. For one of these, Fig. 5, the preceding stimulus had a reënforcing effect for all the intervals beyond 1.8 sec. The second trough in the wave or interval of maximum inhibition was at either 2.64 sec. or 2.92 sec., except for the person for whom there was constant reënforcement beyond 1.8 sec., in which case the first interval of least reënforcement or second trough was at 3.19 sec. This was the second interval of greatest enhancement, or second crest, for four of the others. Then followed a third point of no effect or inhibition, which was 3.75 sec. or 4.03 sec. For the person for whom the preceding stimulus had least enhancing effect at 3.19 sec., the second interval of greatest

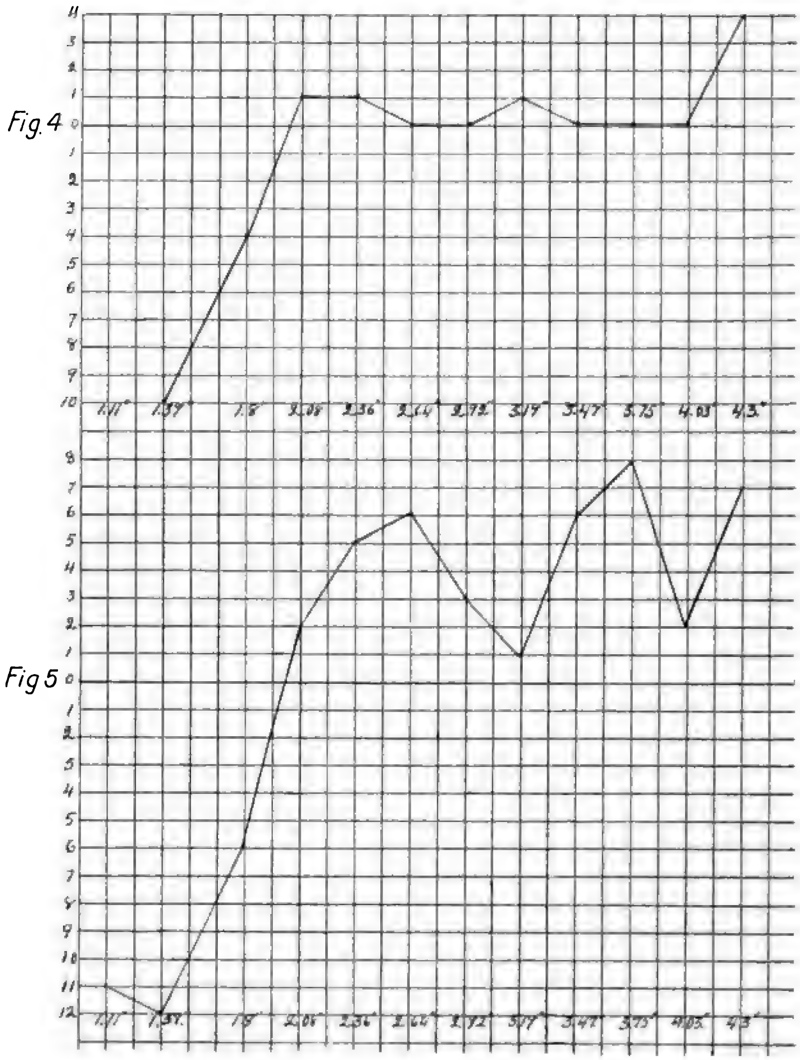


Plate IX.

other series. Thus the fourth member of each series was at all times fixed and constant while the preceding stimulus occupied successive progressive positions round the disc. The other members of each series were chosen at random, care being taken that the fourth figure was not duplicated within its series, since it would then have taken part in inhibition within the series.

By adjusting the front disc, I exposed any one of the series desired, and by removing the cardboard blind from one of the suggestion slits, I gave a stimulus at the desired time-interval in advance of the fourth member of the series. The first interval I used was 1.11 sec. as Miss Kleinknecht had tried intervals up to 1 sec. My second interval was 1.39 sec., the third 1.8 sec., and then every .277 sec. up to 4.3 sec. In performing the experiment I exposed alternately a series without and a series with a preceding stimulus—taking from the observer three reports of each—until the six series had been seen. I then repeated this, exposing with a preceding stimulus those series that had been exposed without preceding stimulus, and without preceding stimulus those series that had been exposed with a preceding stimulus in the first instance. In this way I equalized and minimized the effects of novelty and memory.

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reënforcement coincided with the interval of greatest inhibition for the majority of the other observers. For four of the six observers, the third interval of greatest reënforcement was 4.3 sec. In this, the observer agreed for whom the last interval of greatest reënforcement was 3.75 sec. Thus while, for this observer, the first two points of greatest reënforcement were separated by an interval of 1.11 sec., the second and third points were separated by an interval of only .55 sec. This same thing occurred in the records of two other observers, for one at this point, and for the other at another point. Of the two dissenters from the opinion of the majority that the third crest was at 4.3 sec., one was an erratic observer; and for the other, there was a slight reënforcement at 4.03 sec. and no effect at all at 4.3 sec.

Fig. 1 represents the average of the records of the six observers. The curve is based on the difference between the number of times the fourth members of the series were seen with and without preceding stimulus. The base-line represents the number of times the figure was seen without preceding stimulus, taken each day as the normal for that day. Figures above the base-line represent the greater, and those below the line, the less number of times the figure was seen with preceding stimulus, or reënforcement and inhibition, respectively. The first two points are the average of fifty-four observations; each point beyond the second is the average of 108 observations. Figs. 2, 3, 4, and 5 represent individual records constructed as Fig. 1, each point being the average of eighteen observations.

The curve in Fig. 1 is somewhat misleading in showing points of maximum reënforcement at 3.19 sec., 3.75 sec., and 4.3 sec. In no individual case was this true. The reason for the crest at 3.75 sec., or at least for its height, is that in two cases reënforcement was considerable at this interval, and there was little inhibition to offset this in the general average. At 3.19 sec., which was the second interval of greatest reënforcement, for four out of the six observers, owing to practice, the reënforcement was not great (Fig. 4), but in no case was there inhibition at this point. Thus for the lack of strong positive effect at 3.19 sec. and the lack of strong negative effect at 3.75 sec., the two crests are the same height, while the first represents the maximum effect for four and the second for two observers.

From these results, taking everything into consideration, my conclusions are:

(1) If a stimulus precedes at various time-intervals its duplicate

in a series of stimuli, it will alternately inhibit and reënforce the perceiving of the duplicate stimulus.

(2) Within 4.5 sec. there are at least three points each of maximum inhibition and maximum reënforcement.

(3) The points of maximum inhibition and likewise those of maximum reënforcement are separated by intervals of from .55 sec. to 1.2 sec. — more often by one of the two extremes than by any mean.

(4) Up to 4.5 sec., as the time-interval increases, the maximum inhibition generally decreases, while the maximum enhancement correspondingly increases.

What the limit of this periodic effect is, I cannot as yet say, as up to the present I have not used time-intervals beyond 4.3 sec. But from the intensity of the effect at this interval, I do not expect the limit to be within several seconds.

THE INTERFERENCE OF OPTICAL STIMULI

BY H. KLEINKNECHT

The purpose of this investigation is the determination of the location, extent, nature, and cause of the interference of optical stimuli. Ranschburg[1] studied the phenomena carefully in using optical stimuli which were spread over the retinal field, for instance, a series of letters or figures one beside the other. But if we are to experiment on the inhibitory influence of a certain qualitative impression, we must try to eliminate the local difference; the letters or figures ought to be seen at the same spot.

This became possible by a stroboscopic arrangement, consisting of two parallel circular discs one foot apart on the same axis, whose motion was controlled by an electric current.

The discs were 60 cm. in diameter. Thirty-six radii were drawn equidistant on the farther disc, and on these were clasped black tin strips bearing letters or numbers or colors. The nearer disc was similarly divided and an opening, 3 mm. in width, was cut at each radius. This exposed the number. A cardboard placed between the discs limited the range of vision, its opening being 4×5 cm.

The figures were 10 mm. high, white, and placed on a dark background.

Preparatory stimuli were given to enable the subject to adjust his eye to the farther disc. They were so placed as to fall on different retinal points, thus avoiding fatigue.

Many of the tests employed by Ranschburg were used again to ascertain the influence of the change in method and with the hope that such differences might throw some light on the nature of the interference. At first there were six subjects, afterwards eight — all graduate students and trained in laboratory work. The experiment was carried on in the morning. Numbers consisting of six digits were exposed on a dark background. The time of exposure varied with the subject, but was constant throughout the experiment. The subject

[1] Ranschburg: Zeitschrift für Psychologie, vol. 30, 1902.

was asked to record the number immediately after perceiving it, but in almost every case it was read verbally (its retention being thus facilitated) and then recorded.

For the first few weeks letters were used. But since subjects found it very difficult to distinguish these, a change was made to figures. For a month and a half numbers were given for the purpose of training the subjects and of ascertaining the speed best adapted to each. This varied from 5½″ to 8″ a revolution, each figure being exposed from 115 to 166 sigma.

Three series of numbers were given: (1) Homogeneous, containing a repeated figure, as, 495851. (2) Heterogeneous; as, 708654. (3) Similar, that is, in construction; as, 813470 (8 and 3 being easily substituted for each other). Other similars given by Ranschburg are 9 and 0, 9 and 6, 9 and 2, and 5 and 3.

In order to determine the place of greatest interference, the repeated figures were located in all possible positions, while the preceding and succeeding figures were left unaltered, so as to obviate any new influences which might result from a change of relations. There are fifteen possible variations of the series: *mabcdm*, *ambcdm*, *abmcdm*, *abcmdm*, *abcdmm*, etc.

The following table, illustrative of the scheme *ambmcd*, will show the character of the results obtained. Only the numbers in which errors occur are here recorded, those figures which were incorrectly perceived being printed in heavy type. The dash is used when the location of the figure omitted is known, and the interrogation mark when the reply is doubtful.

	8″ V.	8″ R.	5½″ S.	5½″ M.	5½″ H.	8″ E.
708025	70625	76082	......	70825	7082—5	70285
958564	95584	......	......	95864	985 ? 4	958—54
281845	281485	......		20861	28185	281-54
436392	43632	43636	436932	436924	43632	43632
526273	526723	5257	572673	52763	......	52623
940469	94069	940465	......	94069	94640	940—69

The interference may result in permutation, substitution, or inhibition. The latter two may take several forms; as, inhibition of identicals, of similars, of dissimilars, the location of the omitted figure being known or unknown; also, substitution of an identical, similar, or dissimilar figure which precedes or follows.

The homogeneous series (540 tests) gives results as follows:

Homogeneous Series

	Location of the Identicals.	Inhibition of Identicals. Spot Known	Inhibition of Identicals. Spot Unknown	Inhibition of Similars. Spot Known	Inhibition of Similars. Spot Unknown	Inhibition due to Location. Spot Known	Inhibition due to Location. Spot Unknown
	mabcdm						
1	1=6		1	1	4		5
	ambcdm						
2	2=6		5		2	1	3
3	3=6		2		3		5
4	4=6		3	3		4	2
5	5=6	6	15(?)		4		5
6	1=5	1	2	1	2	1	3
7	2=5		5		3		3
8	3=5	3	7			2	1
9	4=5	2	21 (?)		2	5	3
10	1=4		9		4		3
11	2=4	3	9		4	1	7
12	3=4	3	19 (?)		1	2	4
13	1=3		†5+3(?)		2	1	10
14	2=3	1	11 (?)		3		8
15	1=2		6(?)	1	4		9
Total		19	48+75 (?)	6	38	17	71

† Fusion or inhibition?

I. *Inhibition*

(1) There is considerable inhibition only when identicals are next to each other.

(2) There is but little difference in the amount of inhibition when identicals are removed two and when removed three places.

(3) The interference is greatest when 3d = 4th, 4th = 5th, and 5th = 6th figures, in which schemes it is almost equal in amount.

(4) When identicals are adjacent, it is impossible to decide whether there be inhibition or fusion, *i. e.*, whether one be inhibited and the other appear, or whether the figure seen be a fusion of the two (unless there is an omitted figure whose location is known to the subject). Its intensity does not serve as a clue, for the perception of the number demands the full concentration of the attention.

II. *Substitution*

When the interference is not sufficiently great to cause inhibition, substitution may result.

(1) In the majority of cases the substituted figure is a dissimilar not occurring in the number.

(2) A preceding figure is frequently substituted.

(3) Occasionally a figure is replaced by its similar, but this is not true of the homogeneous element. (Cf. with Ranschburg.)

(4) Sometimes the next figure in the natural number series is substituted; as, 9 for 8, 6 for 5.

(5) The figures containing straight lines (4, 7, and especially 1) are less subject to illusion; likewise the smaller numbers (1, 2, 3, 4).

III. *Permutation*

The permutation represents the least interference.

(1) The 4th and 5th figures are most often exchanged.

(2) The figure is seldom permuted more than two places, and generally but one.

The recording of the number was most interesting. Generally the first few figures and the last were written without comment, but the 4th and 5th often called forth an expression of doubt, which was immediately followed by an exclamation at the coming of the figure into consciousness as if by "inspiration." The experience was extremely peculiar. The figure, fully as distinct as those already perceived, was always from 5″ to 10″ late, and seemed to "pop in unannounced" — to "come from nowhere." A substitution or permutation occurred without this lapse of time.

Heterogeneous Series

(1) There are less than half as many inhibitions as in the homogeneous series, the largest number being in the 4th and 5th places.

(2) The number of substitutions is decreased by a fourth, the identicals and similars remaining the same.

(3) There are no fusions.

(4) Fewer permutations are found in this series. The 4th and 5th figures are most often permuted. In a very few cases the figure is permuted four and five places.

(5) There are an equal number of doubtful perceptions in both series.

Similar Series

(1) There are few cases of inhibition, and even more surprising is the small number of cases in which a figure is inhibited by its similar.

(2) There are more substitutions, 6 being very often substituted for 5, generally in the 6th place and when preceded by 0 or 9, often by both. Similars are never replaced by identicals (69 by 66 or 99) as Ranschburg found in his experiments.

(3) The fusion of similars equals that of identicals in the homogeneous series.

(4) The number of permutations is the same as in the homogeneous series and less than in the heterogeneous.

(5) The doubtful perceptions have decreased by half.

That there are fewer errors in this series than in the homogeneous or heterogeneous, may be due to the fact that it was given last, especially since one subject showed marked improvement in the entire series and another during the last half. These subjects suddenly began to see six figures, while previously they had seen but five and those contained errors.

In the above 1620 tests, 9 and 0, and 8 and 3, are sometimes inhibited by and substituted for each other, but the remaining similars mentioned by Ranschburg seldom have any such effect.

It is impossible to determine definitely the nature of the interference, the greatest uncertainty existing in the homogeneous series when two identicals are adjacent. But the interference is dependent not only upon the identity or similarity of the figures of which the number is composed but also upon their location.

INHIBITIONS

	1		2		3		4		5		6		Total	
	Place known	Place unknown	K.	U.	K.	U.	K.	U.	K.	U.	K.	U.	K.	U.
Homogeneous			1	13	2	28	9	41	15	31	15	44	42	157
				6 (?)		14 (?)		19 (?)		21 (?)		15 (?)		75 (?)
Heterogeneous	2	5	1	8	2	15	4	36	5	34	2	25	16	123
Similar				4		4		5	3	6	2	13	5	32
Total excluding (?)	2	5	2	25	4	47	13	82	23	71	19	82	63	312
Total of Known + Unknown	7		27		51		95		94		101		375	

(?) Inhibition or fusion.

SUBSTITUTIONS

	1	2	3	4	5	6	Total
Homogeneous	8	12	26	27	38	14	125
Heterogeneous	4	4	14	21	30	20	93
Similar	1	5	9	25	33	27	100
Total	13	21	49	73	101	61	318

FUSIONS [See (?) under Inhibitions]

	1	2	3	4	5	6	Total
Homogeneous		1	2	1	3	10	17
Heterogeneous			0		0	0	0
Similar			3	3	6	6	18
Total		1	5	4	9	16	35

Note. There were no clear cases of fusion, but the evidence favored fusion rather than inhibition.

PERMUTATIONS

		1	2	3	4	5	6	Total
Homogeneous	(a)	6	29	46	56	30		167
	(b)		5	21	45	68	35	174
Heterogeneous	(a)	15	25	60	62	28		190
	(b)		14	23	51	82	44	214
Similar	(a)	13	20	37	78	16		164
	(b)		12	17	26	75	28	158
Total	(a)	34	74	143	196	74		521
	(b)		31	61	122	225	107	546

(a) forward, (b) backward

Note. The permutation of an inhibited figure was not noted unless its location was known; hence the difference in the number of forward and backward permutations.

	1	2	3	4	5	6	Total
Total Interferences	54	160	323	509	524	300	1870
%	3%	9%	17%	27%	28%	16%	
Absolute Errors (excluding Permutations	20	55	119	191	225	103	713
	3%	8%	17%	27%	31%	14%	

ABSOLUTE ERRORS (excluding Permutations)

	Homogeneous	Heterogeneous	Similar
Inhibitions	199	139	37
Substitutions	129	93	101
Fusions	17		18
(?)	75		
Total	420	232	156
	52%	29%	19%

Over 50 % of the errors were found in the 4th and 5th places.

[Ranschburg: 90 % of errors in right half — 60 % in 5th place, 30 % in 4th, few in 6th.]

In 1620 tests, the homogeneous series contained 52 % of the absolute errors, the heterogeneous 29 %, and the similar 19 %.

Colors

In the hope that some light might be thrown upon the main question at issue, the writer changed the stimuli, using colors instead of numbers.

It was important that the colors should be of the same or only slightly varying intensity and that they should be easily distinguishable. In a series of preliminary experiments in which red, blue, yellow, green, brown, gray, pink, and violet were used, red was lost in 8 % of the tests, and gray in 25 %.

Colors 1×4 cm. in size "ran into each other," while those which were 1×1 cm. remained distinct.

Here it was found necessary to distinguish between the various factors which might cause inhibition. Three factors entered into each test — perceiving, naming, remembering.

Four subjects found difficulty in naming, especially at first. The various methods of naming are given below in detail. M. says: "The name of the color is localized in my mouth. Generally there is no movement of the tongue — an impulse only; and the name is felt in that part of the mouth where the sound would be reflected, as, red in the upper part, blue near the front, etc."

S.: "Usually there is no apparent tendency to pronounce. Occasionally, naming them over inaudibly before recording is found advantageous."

E., V., and H.: "The naming is mental, but is accompanied by a slight movement of the tongue and throat."

684 heterogeneous and 200 homogeneous tests showed that greatest inhibition occurred in the following order: 4th place (27 %), 3d (26 %), 5th (24 %), 2d (11 %), 6th (8 %), 1st (4 %). There was but little difference in the 3d, 4th, and 5th places.

During first tests subjects were allowed only one exposure, but later it was thought best to eliminate all omissions resulting from inability to name colors perceived, and hence they were asked to record only when able to name all colors perceived during that exposure. However several required but one exposure.

Preliminary drill was given for two weeks. Since no clear cases of fusion had been obtained in the entire number-series, the one aim of the experimenter was to ascertain whether fusion of colors, even though of heterogeneous, be possible. Eight hundred heterogeneous tests gave 927 cases of inhibition, 7 of fusion, and 18 which, though somewhat doubtful, yet gave more evidence of fusion than of inhibition. Yellow (3d place) and brown (6th place) were seen as yellowish-brown, brown and pink as pinkish-brown, etc. Gray was seen several times instead of a color and its complementary when these were in immediate succession. This was true of both red and blue. Half of the total number of substitutions was due to the displacement of yellow by brown. And a color not in the series was as likely to be substituted as one preceding or following the displaced color.

Two hundred and fifty-two homogeneous tests showed that there is greatest interference when identicals are in immediate succession, and least, when removed two places. The doubtful (fusion?) cases number *one third* of the inhibited. The 4th and 5th colors are permuted most often, as was found to be the case in the heterogeneous series also. The element is generally permuted but one place.

The heterogeneous color-tests show three times as much interference as the corresponding number-tests, and the homogeneous twice as much. The discrepancy in the amount of variation may be due to the experiments with the heterogeneous colors being earlier, when naturally more errors would be made.

However, a comparison of 252 homogeneous with the same number of heterogeneous tests, taken at the same time, shows that there is a much larger difference in the number of absolute errors between the heterogeneous and the homogeneous number-series than there is, proportionately, between the two series of color-tests.

Lest the want of correspondence in the results might have been due to the comparatively small number of immediately successive identicals in the color-tests, 90 homogeneous tests, equally distributed among all possible variations in the location of the identical elements, were compared with 90 heterogeneous, and it was unexpectedly found that the absolute errors as well as the permutations were almost equal in the two series. Nevertheless, the validity of a conclusion based on so few tests may well be questioned.

Ranschburg found that simultaneous homogeneous stimuli interfere with one another; while simultaneous heterogeneous stimuli clear the way for one another. On the basis of the experiments with

numbers, the writer would amend the conclusion reached in the earlier research to read thus: Homogeneous optical stimuli, whether occurring simultaneously in different positions, or in immediate succession in the same positions, interfere with one another; while heterogeneous stimuli clear the way for one another.

SUBJECTIVE AND OBJECTIVE SIMULTANEITY

BY THOMAS H. HAINES

THIS investigation finds its starting-points in two widely separated lines of experimentation in the problems of attention. These two lines are the "scope-of-attention" experiment with the tachistoscope, and the "time-displacement" experiment with the pendulum apparatus. It seems to me these two can be brought into relation to each other to the help of each of them individually, and that an investigation taking these wide relations within its scope may reasonably be expected to throw new light upon the manner in which mental processes are related to each other when they are together in consciousness at the same time. The first of these experiments (tachistoscopic) is concerned with the number and relative clearness of the processes which go on at the same time. The second (displacement) is concerned with the conditions of the subjective displacement of one of two objectively simultaneous stimuli with reference to the other. Its problem is the essential psychological problem involved in the astronomer's error in transit observations by the eye-and-ear method, for the personal equation arising in these observations is more a matter of the reciprocal relations among the processes which are together in consciousness at the moment of observation than it is of mere reaction time. It is primarily more a matter of relative clearness, as controlled probably through interference of one with another, than it is of the more or less temperamental facility of converting ideas into action.

The psychological question at the heart of the observation-error, called the personal equation, is this, — What are the conditions which hinder such a division of attention among the parts of the complex operation of coördinating sense-stimulations, that the processes which start simultaneously may proceed to equal clearness at the same time, and so be perceived as simultaneous? The facts sought in order to answer this question are the very same as some of those, at least, demanded by the "scope-of-attention" investigation when it really opens up to its true problem. W. Wirth [1] has recently shown, in an exhaustive criticism of the tachistoscopic method, that "scope of attention" is primarily concerned with the *relations* of

[1] W. Wirth: Zur Theorie des Bewusstseinsumfanges und seiner Messung, Philos. Studien, vol. 20, p. 487, 1902.

the processes present together, and that this demands a previous exhaustive study of their *relative clearnesses*. Earlier studies by the tachistoscopic method, as, for example those of Cattell [1] on the relatively short time for the perception of letters in words, as compared with that for separate letters, and the overlapping of processes in continuous reading, showed that the important question is, *what* are the processes which may go on at the same time. Leaving out a statement of the nature of the processes is equivalent to leaving out one of the dimensions when endeavoring to state the contents of a solid. The scope of attention can be defined adequately only when one knows fully *what* the separate processes are as well as *how many* there are. This analysis, which the scope-of-attention problem demands, cannot fail to be directly fruitful for the solution of the time-displacement problem. The analysis of this larger problem directly involves the former. Any attempt to investigate the time-displacement of sense-impressions from simultaneous stimuli must inevitably place the highest value upon the whole detailed analysis of any moment of attentive effort.

The present investigation, starting with the facts of time-displacement, and taking the hint offered by Gonnessiat,[2] attempts to show, by a more complete analysis, the effects of the various relations within each series, — the visual within which the sounds are to be placed, and the auditory series itself, and also relations existing between the two series. In other words, the attempt is made to strip the "displacement" experiment until nothing more remains to be coördinated than a single pair of simultaneous stimuli. This was the experiment of Exner.[3] He investigated the shortest discriminable interval marked off by various pairs of stimuli, addressed to the same sense and to different senses. From this coördination of a visual and an auditory stimulus, where the limits of the "specious present" are obtained, I make a turn into the realm of the scope of attention. By a new method, whereby impairment of accuracy of processes is made the test as to whether the processes have proceeded together, it is shown that two such perceptual processes can go on just about as well at the same time as separately. Since this test is subject to the objection that the visual and auditory processes may

[1] Cattell: Ueber die Trägheit der Netzhaut und des Sehcentrums, Philos. Studien, vol. 3, p. 94, 1886.

[2] Gonnessiat: L'Equation personnelle, Paris, 1892.

[3] Exner: Experimentelle Untersuchungen der einfachsten psychischen Processe, Archiv. f. d. gesammte Physiologie, vol. 7, p. 601, 1873, and vol. 11, p. 581, 1875.

really be successive, though seemingly at the same time, owing to retinal inertia, the same question is removed to an entirely different plane in a further and more detailed set of experiments where the processes combined are *judgments of comparison* based upon one and the same visual sensation.

Experiments in Time-Displacement

The Leipsic Complication Experiment with the pendulum apparatus (for description of this see Wundt's Physiol. Psy., 5th ed., vol. 3, p. 82) was an early adaptation of the astronomers' eye-and-ear method to the purposes of psychological experimentation. Instead of localizing a visual stimulus (star on meridian) in an auditory series (clicks of a chronoscope) as in the eye-and-ear method, this adaptation localized an auditory stimulus (bell-stroke) in a visual series (successive positions of a pointer on a graduated circle). This pointer passed around to the right and to the left from the position of rest, in which it pointed vertically upward, as the pendulum, to which it was connected by clockwork, swung back and forth. By a simple adjustment the bell-stroke could be made to come at any point in the complete double swing of the pendulum, and so anywhere in the arc over which the pointer moved. This machine makes an additional problem as to the effects upon displacement of the increasing and decreasing speed. My aim being to simplify as much as possible the displacement-error and so reduce it to its elements, this feature was not only not of direct interest, but it was very desirable to dispense with it altogether. This was done by arranging the visual series so that the members were shown in perfectly regular order, *i. e.* with equal time-intervals, throughout the series. These equal intervals were secured by the rotation of a disc at a uniform rate.

My method also gave a more distinctly serial character to the visual stimuli, in that they were separated by blank periods. The series consisted of letters in alphabetical order. Denison's smallest white letters, about six millimetres in height, were pasted upon a disc of black cardboard, near the circumference and perpendicular to radii, so that they would appear in succession and right side up, to an observer looking through a slit at the peripheral region of the disc, as it rotated. The letters were placed in three concentric rows, so that as the disc rotated they appeared in three different places. The disc was 56.5 cm. in diameter. As a further aid in securing separate exhibitions of letters, another black disc of the same size as the one bearing the letters, with radial slits 2 mm. wide and cut in

from the edge 4 cm., opposite each letter on the other disc, was mounted on the same shaft, six inches from the first, and between it and the observer. A short observation-tube was placed at the same height as the axis of the discs parallel to this axis, and opposite the slits when they were at this elevation. Looking through this, as the discs were rotated, one would see the letters right side up and in serial succession. Uniform illumination was secured by working in a dark room with artificial light. An electric lamp was hung between the discs. Uniform motion was secured by an automatic control gravity motor, connected by belt with a pulley on the disc-shaft.

The auditory stimulus, a click, adjustable to any part of the series, was made as follows: A wooden shaft, mounted on the same axle as the discs, and beyond the discs from the observer, could be rotated freely around the axle when the nut securing it was loosened. This shaft extended beyond the edge of the disc. It carried a copper wire which was in contact with the axle. A mercury cup was placed on the table, upon which the machine rested, in such position that the copper tip passed through the mercury when the discs rotated. It was thus a very simple matter to connect an electric sounder so that it would click every time the circuit was made by the copper passing through the mercury. And, by the adjustment of the wooden shaft, the click was readily placed anywhere in the visual series.

As already suggested above, the length of interval between members of the visual series, and also the time between clicks, seem to be important factors in determining the amount, and perhaps also the direction of the displacement. Bessel found his personal equation was considerably diminished when he used a clock marking half-seconds instead of one marking seconds. Wolf also diminished his error by using a clock beating one hundred times a minute instead of one beating seconds, which he was accustomed to use. Wundt found his customary negative displacement on the pendulum apparatus (coördinating the sound with a position of the index earlier than that with which it was actually simultaneous) disappeared when he had members of the visual series one thirty-sixth second apart and the auditory stimuli one second apart. It seemed important at the outset, therefore, to determine, if possible, the effects of each of these factors.

Both Intervals Progressively Varied

In each experiment the observer was allowed to observe as many complications (coincidences of click and letter) as he desired, in order

to assure himself of his judgment. The experimenter counted and recorded the number obseved in each experiment. Experiments were made in series of ten. Six different combinations of intervals were used in this first group of experiments. The auditory intervals (time between successive clicks) and visual intervals (time between successive members of the visual series) are given at the tops of the columns in Table I. This table is a summary presentation of the results of this group. There were three observers. During each hour of experimentation with a given observer, at least one series with each of the first four time-interval combinations was tried out. "Aver. num. Trials" means the average number of complications observed in the whole number of tests averaged. "Num. Series av." means the number of series of ten experiments each averaged to give the displacement results below. "Aver. Error" is the average of all the displacements of the auditory impression, *irrespective of the direction of the displacement.* "Mean Displacement" is the *actual mean displacement* as obtained by dividing the algebraic sum of all displacements, positive and negative, by the number of experiments. The plus sign indicates a positive displacement, and the minus sign, a negative. Negative and positive are here used in the sense customary in similar experiments, — namely, the click, being heard as simultaneous with a visual impression which actually came before it, was said to be displaced negatively, and the click, being heard as simultaneous with a visual impression coming in fact later than it did, was said to be displaced positively. Average errors and mean displacements are given in the table in thousandths of seconds. Observers were asked to locate the click in the visual series in terms of one tenth the distance or time between the letters.

TABLE I

Obs.							
	Aud. Interval	1.28 sec.	2.56 sec.	4.04 sec.	8.40 sec.	1.28 sec.	2.02 sec.
	Vis. Interval	.040 sec.	.080 sec.	.120 sec.	.260 sec.	.080 sec.	.120 sec.
B	Av. num. Trials	13.9	5.8	3.8	2.1	9.8	13.9 sec.
	Num. Series av.	8	13	13	8	2	2
	Aver. Error	.056 sec.	.064 sec.	.077 sec.	.164′ sec.	.045 sec.	.067 sec.
	Mean Displac'mt	+.045 sec.	−.040 sec.	−.067 sec.	−.152 sec.	+.045 sec.	+.067 sec.
Bo	Av. num. Trials	9.4	4.1	3.0	2.0	5.5	3.5
	Num Series av.	6	10	11	8	3	2
	Aver. Error.	.114 sec.	.060 sec.	.054 sec.	.049 sec.	.05 sec.	.082 sec.
	Mean Displac'mt	+.114 sec.	+.045 sec.	+.033 sec.	.000	+.045 sec.	+.082 sec.
M	Av. num. Trials	6.3	3.1	2.5	2.2	5.3	4.4
	Num. Series av.	9	12	12	10	3	2
	Aver. Error	.09 sec.	.07 sec.	.076 sec.	.110 sec.	.067 sec.	.172 sec.
	Mean Displac'mt	+.089 sec.	−.058′ sec.	−.058 sec.	−.104 sec.	+.062 sec.	+.168 sec.

The first four combinations of intervals above, with which the major part of the results was obtained, it will be noticed, are approximately proportionate increases in each interval, column by column. These conditions were planned with a view to revealing the conditions, most favorable for coördinating the auditory and visual impressions, for each observer, so that his displacement would disappear, or show a tendency to disappear. So far as is shown by these results, there are here two types of observer. Bo has no mean displacement for the 8.40 — .260 sec. combination, and it steadily decreases toward this point as the two intervals increase. Both B and M, on the other hand, have a considerable positive mean displacement for the 1.28 — .040 sec. combination, and a considerable negative mean displacement for the 2.56 — .080 sec. combination, and there is a further increase in the negative displacement as the intervals increase from this point. It seems as though these observers would give a mean displacement of zero for some combination of intervals between these first two. It will be noticed that the average number of trials is exceptionally large for all three of the observers in the first combination. This seemed to be pretty clearly due to the very short interval separating visual impressions.

The Auditory Interval *alone* Varying

In order more certainly to isolate the influence of the time-interval between successive auditory impressions, another series of experiments was performed, in which this interval between clicks, alone, was varied from series to series. The visual interval was kept at .083 sec. throughout. This seemed to be about the shortest time-separation at which the successive impressions were perfectly distinct. The auditory impressions were at 1, 1½, 2, 3, and 4 sec. intervals. The additional observer, H, was myself. I obtained these results by experimenting alone. I adjusted the wooden shaft carelessly to a new position and started the machine. When speed was attained, I would make the observation just as an observer for whom the adjustment had been made. I would have as little idea beforehand as he with regard to the position of the click in the series of letters. Having made the observation, however, I measured the actual place of the sound and recorded it, as well as my judgment. In this way, of course, I had some idea, all the time, as to what kind of displacements I was making and how large. I was as careless of this knowledge as possible, and the records were laid aside absolutely, until I was through with the whole experiment. Terms used in Table II are the same as in Table I.

TABLE II

Obs.						
	Aud. Interval	1 sec.	1½ sec.	2 sec.	3 sec.	4 sec.
	Vis. Interval	.083 sec.	.083 sec.	.083 sec.	.083 sec.	.083 sec.
B	Av. num. Trials	8.5	6.8	6.2	4.8	4.8
	Num. Series av.	10	10	10	10	10
	Aver. Error	.097 sec.	.108 sec.	.106 sec.	.097 sec.	.101 sec.
	Mean Displacement	+.097 sec.	+.108 sec.	+.106 sec.	+.097 sec.	+.101 sec.
Bo	Av. num. Trials	6.0	5.0	4.2	3.2	3.1
	Num. Series av.	10	10	10	10	10
	Aver. Error	.103 sec.	.080 sec.	.081 sec.	.092 sec.	.082 sec.
	Mean Displacement	+.102 sec.	+.073 sec.	+.078 sec.	+.089 sec.	+.075 sec.
M	Av. num. Trials	4.4	3.8	3.4	3.0	2.8
	Num. Series av.	10	10	10	10	10
	Aver. Error	.088 sec.	.084 sec.	.081 sec.	.068 sec.	.052 sec.
	Mean Displacement	+.086 sec.	+.079 sec.	+.072 sec.	+.051 sec.	+.048 sec.
H	Av. num. Trials					
	Num. Series av.	10	10	10	10	10
	Aver. Error	.043 sec.	.036 sec.	.047 sec.	.040 sec.	.037 sec.
	Mean Displacement	−.022 sec.	−.012 sec.	−.027 sec.	−.017 sec.	−.013 sec.

One series of ten of each of these combinations was given during each hour of experimentation with each observer. These were also given in a different order each day, so that no combination should have the advantage, by practice or lack of fatigue, in the average of the ten series. Here again it was evident, in the records of each of the observers for whom the count was made, that the largest number of trials was necessary in the 1 − .083 sec. combination. It thus appears that it was not the short visual interval, .040, in Table I, that was responsible for the large number of trials necessary in the first combination. Here, where there is the same visual interval of .083 sec. throughout, it must be the short auditory interval which makes particularly difficult conditions for attention. This agreement between the results in both groups of experiments seems to indicate unfavorable conditions for accurate coördination at auditory intervals as short as one second. The large changes in the mean displacement for B and M between the first two combinations in the first group (Table I) was kept especially in mind in planning this second series of combined intervals. It was presumed from the results given by these observers in Table I that they would each, with the range of auditory interval presented them in these experiments, show a point of no displacement, or a very slight one, and an increasing displacement on each side of this point. They both seemed to indicate a time-interval favorable for the "ripening of appercep-

tion" as Wundt and Von Tschisch call it, and I planned these experiments especially to bring it out more clearly. But there is far less indication of a time most favorable for "ripening" than in the previous group of experiments. B and M both give all mean displacements as positive, and decidedly small differences in displacement for the various combinations. Results of Bo are, however, entirely consistent with those of Table I. H gives a very small negative mean displacement throughout. This, as well as the smallness of the average error, may be due to the knowledge of results which I had.

An examination of the detailed daily results, which cannot be exhibited here, shows considerable change in the direction of the displacements as the work proceeded. This is especially marked in the case of B, who, during the first two hours of experimentation, gave only negative displacements. Through the rest of the first group there was a gradual increase of positive displacements, and in the last two hours about 90% were positive. In the second group he did not give a single negative displacement. The same change is manifested in the results of M for the first group; but he did not change over nearly so completely. In the five hundred experiments of Table II, for M, there are three hundred and ninety-two positive, sixty-seven negative, and forty-one *no* displacements. Bo gave a number of positive displacements from the start. These increased considerably in the second over the first group, showing only thirty-seven negative displacements in the second group. This change in the direction of the displacement, rather independently of the intervals, is an interference with the main purpose of the experiment. It may represent the effect of practice.

Angell and Pierce[1] found the same progressive change from negative to positive displacements. They explained it as a change in the focus of attention. The visual series is focal at first, and the sound becomes focal in later experiments. Negative displacements result from fixing the last possible point in the visual series before the sound is heard, while positive displacements result from getting the first letter possible after the sound. The method of my observers, with the large numbers of trials at their disposal, was to "let the sound *announce* the letter" on the first trial, and then to "lie in wait for the letter" so announced, and to "see whether it was too late or too early." It was found to be too late usually, for this was the second method of Angell and Pierce, which gave positive displacements.

[1] Angell and Pierce: Researches upon Attention, American Journal of Psychology, vol. 4, p. 528.

So at the next trial the preceding letter would be waited for, and tested in the same way. The first trial was thus auditory-visual attention and the second was visual-auditory, and there was a striving after a balance where neither auditory nor visual impression had the preference.

As soon as adjustment to the conditions of a given combination had been secured, it was a simple matter to anticipate, with a fair degree of accuracy, both a given letter and the recurrence of the sound. The attention could thus be pretty accurately divided between the two, and a very small time-displacement was the result. When I was acting as observer, a change of the auditory interval *upset* the whole *plan* of procedure for a short time. I had to accustom myself to the new rhythm. But as soon as this adjustment was made, it was just as easy to make the judgment at one rate as at another, barring variations which might be called fortuitous, since they were so small. This experience with the conditions here under consideration, as well as the introspections of the other observers, convinces me that the conception of an apperception-ripening time has been overworked.

It is true that I find here, just as Pflaum [1] found, displacements in both directions with every observer. It seems very doubtful to me, however, whether these are in any sense due to what may be considered a fixed apperception-time for a given observer, under fixed objective conditions. The facility with which adaptation is made to the changed conditions of a new combination of intervals, so that just as small displacements are made under one as another, indicates to my mind that one can control the conditions so that the apperception shall ripen quickly or slowly, depending upon the warmth of the interest, and the concentration or division of the attention, — that there is a capacity in the ordinary individual so to adapt himself to the conditions as to do equally good work in coördinating two sense-impressions anywhere within a wide range of intervals. The influence of the length of the interval separating succeeding clicks, in determining displacements, has been considerably overestimated. I should state here that no one of the three observers had any specific training to reduce the displacement. The results were not discussed with them. They had no means of knowing what displacements they were making. This certainly adds strength to the inference, from these results, that there are adaptable apperceptive conditions for coördinating sense-impressions.

[1] Pflaum: Neue Untersuchungen u. d. Zeitverhältnisse der Apperception einfacher Sinneseindrücke, Philos. Studien, vol. 15, p. 139.

The Influence of the Length of the Series of Visual Impressions

The next step in the analysis of the complication experiment, bringing it into relation with the simple coördination of two disparate stimuli, is to show, if possible, the influence of the *series* of *visual* impressions. This naturally divides into two lines, namely, (1) the *length* of the series as such, and (2) the relative influence, in case of a given kind of displacement, of the part of the series coming *after* the auditory stimulus, and the part *preceding* it. For the first, I used in comparison, a series of twelve letters, a series of three, and a single letter. For the second, the letter, whose coördination with the click was set as the task of the observer, was made successively the first, the last, and the middle member of a series of five letters.

During each hour of experimentation, the observer was tested as to his accuracy of localization of the click, (1) in a series of twelve letters at intervals of .083 sec., (2) in a series of three at the same interval, and (3) with reference to a single letter. The method for the first two was exactly as in the preceding experiments. In the case of the single letter, he was asked to localize as accurately as possible in terms of the intervals as he remembered them from the series. This introduced an element of uncertainty. One observer, St, would not give any judgments as to time-differences in the case of the single letter. Another method had to be adopted in order to obtain more comparable results. These results (Table III) are presented as showing, by comparison with the following table, the transition from one method to the other. Clicks were at 2-sec. intervals. Each number in the table is the average result of fifty or more experiments. They are in thousandths of seconds, and the plus and minus signs indicate positive and negative displacements.

Table III

Observer	*Twelve Letters*	*Three Letters*	*One Letter.*
A	+.012 sec.	−.029 sec.	−.010 sec.
G	−.022 sec.	+.004 sec.	−.004 sec.
Sh	+.028 sec.	−.079 sec.	−.057 sec.
St	−.050 sec.	−.036 sec.	
Bo	+.029 sec.	−.015 sec.	−.022 sec.

The method of right and wrong cases was used in the next group of experiments, to secure the same conditions of making the judgment in each of the three cases used above. Selecting a letter near

the middle of each series, I asked the observer, in each of these cases, just as in that of the single letter, to say whether the click was before, on, or after the letter. I worked *out*, in successive experiments by successive adjustments, from the position of apparent simultaneity of click and letter, in both directions, to a point where in 75 % of the cases the click seemed to come before; and also to one where it seemed to come after, in 75 % of the cases. So also I worked *in* both ways, by successive adjustments, from regions of clear discrimination of time-difference and direction, to points where the time-relation was uncertain or wrong in 75 % of the trials. By averaging the just perceptible and the just not perceptible, in each case, the thresholds were obtained for "click first" and "click last." The time between these thresholds I call the "range." It is really a measure of James's "specious present" and of Stern's "Präsenzzeit." (An admirable presentation of similar results by Wilhelm Peters[1] has appeared since this work was done.) The best means of comparing these results, for our present purposes, and also of bringing them into relation with the complication-results already obtained, is to take the mean point between these thresholds, and state its position, in time, relative to the time of the visual stimulus (letter) just before or after which the click came. This mean point is called the "Threshold Mean" in the following tables. In Table IV, for example, "After Letter .026 sec." means that the mean point between the thresholds, "click first" and "click last" falls twenty-six sigmas after the time of the exposure of the letter. These results are readily comparable with those of Peters. By dividing the "range" by two, and adding the "threshold mean" to one half, and subtracting it from the other, one has the total interval between "click first" and "click last" and its place with reference to the time of the visual stimulus.

TABLE IV

Obs.		*Twelve Letters*		*Three Letters*		*One Letter*	
A	Threshold Mean	After Letter	.026 sec.	On Letter		After Letter	.020 sec.
	Range		.093 sec.		.041 sec.		.062 sec.
G	Threshold Mean	Before Letter	.015 sec.	Before Letter	.020 sec.	After Letter	.062 sec.
	Range		.072 sec.		.083 sec.		.304 sec.
Sh	Threshold Mean	Before Letter	.003 sec.	After Letter	.027 sec.	After Letter	.003 sec.
	Range		.172 sec.		.111 sec.		.241 sec.

It must be distinctly understood that these "threshold means"

[1] Peters: Aufmerksamkeit und Zeitverschiebung in der Auffassung disparater Sinnesreize, Zeitschrift f. Psychologie, vol. 39, p. 401, 1905.

are not displacements, and that the two cannot be compared as if they were statements of the same facts. These *means* indicate the centre of gravity of the "click first" "click last" interval with respect to the visual stimulus. Changes in this centre of gravity may reasonably be expected to approximate a variation *inverse* to that of the displacements of the auditory stimulus. For example, any change in the conditions which would tend to increase a *negative* displacement would tend also to put the centre of gravity of the "click first" "click last" interval *after* the visual stimulus, or, if it were already after, to increase its time after. So also the *positive* displacement and the position of the threshold mean *before* the visual stimulus may be considered similar indications. For a click given at the time of the threshold mean of a given observer, in connection with the same visual stimulus, would certainly be judged by that observer as simultaneous with the visual stimulus. If, then, this mean is before the visual stimulus, the sound will be displaced positively, *i. e.*, coördinated with a visual stimulus coming later. If the mean is after the visual stimulus, the sound will be displaced negatively, *i. e.*, coördinated with a visual stimulus coming earlier. The position of the mean of the thresholds indicates a tendency toward the displacement of the auditory impression in the opposite direction.

In Table III, three out of five observers, A, Sh, and Bo, show a change from a negative displacement in the series of three to a positive displacement in the series of twelve. If this were the effect of the series, the same should show in the series of three as compared with the single letter. Such a change is manifest in the results of Bo. It is, however, very slight. The others increase the negative displacement from the single letter to three letters. In Table IV, of the same three observers represented, Sh changes the threshold mean from *after* in the three-letter series to *before* in the twelve-letter series, and A changes from *after* in one letter to *on* in three letters. These changes correspond to changes from negative to positive displacements for increase of series and introduction of series. G shows the same change from one letter to three, in both tables. These changes, in 55 % of the cases offered for comparison in the two tables, indicate a *decrease* of *negative displacement* and an *introduction* of *positive displacement* as the *effect* of the *visual series.* The visual element is made more focal in expectant attention as it is more isolated, and so the tendency toward negative displacement and increasing negative displacement as the serial character of the visual impressions is stripped off. But there are strong counter-

active tendencies, which control the 45 % of comparisons not mentioned above, where the increasing series shows increasing negative displacement.

In the series all the observers adopted the method which has been outlined above, that of letting the click pick out the letter, or letting the letter announce itself. One said "the letter hits the sound." After this sorting-out of the letter, they resorted to the system of tests and counter-tests, in succeeding trials, to correct the first impression. One can readily understand, then, that when they were taken off the series altogether, an entirely different kind of adjustment had to be made. G did not succeed in making this new adjustment very well, as is shown by his exceptionally large range under one letter. He could not get the two impressions to come together. In attending to either one, he could not get the other in relation to it. There was something in the visual series which enabled him to get the visual impression in line with the auditory, and when this was absent the same kind of work could not be done.

St had also a peculiar method, which was directly dependent upon the serial character of the visual stimuli and impressions. He allowed the series of clicks and the series of visual impressions to establish themselves as a complex rhythm. Each series was rhythmic independently. The two got connection by means of the click appearing as an "after-strike," as on the piano, to a member of the visual series. The letter "flashes out" for him as that of which the click was the "after-strike." The click was thus between two letters. But there was no amount of before or after about it. It was a general quality of the whole complex which was taken to mean such and such a position of click in the series. What he thus translated into temporal judgments, were qualitative aspects of the rhythmic experience, to which he usually attached no temporal meaning whatever. Learning how so to translate them into temporal terms was a definite process of training for him. Under these circumstances, he of course had an entirely new lesson to learn when the visual series was taken away. In fact, it might be, he would now find no visual impression to which the click could be an after-strike, and so he would be entirely without material to translate into temporal terms.

Under these circumstances it is not surprising to find G and St exceptions to the majority of the observers in this experiment. This makes more probable the effect of the series, inferred above for the other observers, — namely, series decreases negative displacement.

THE INFLUENCE OF THE *Position* OF THE *Series* OF *Visual* IMPRESSIONS

It was noticed in the series of the three letters, particularly, that some observers were much more accurate in their work when the click was near one end of the series. In this experimental group, the comparison is between cases where the click is coördinated with (1) the first member of a visual series of five, (2) the middle member of a series of five, and (3) the last of such a series. The method was the same as that used in obtaining the results of Table IV. H was the letter used in each case for coördination. Results follow in Table V.

TABLE V

Obs.			*H first*		*H middle*		*H last*
A	Threshold Mean	After Letter	.010 sec.	After Letter	.021 sec.	After Letter	.025 sec.
	Range		.072 sec.		.085 sec.		.093 sec.
G	Threshold Mean	Before Letter	.007 sec.	On Letter		Before Letter	.007 sec.
	Range		.124 sec.		.083 sec.		.151 sec.
R	Threshold Mean	After Letter	.032 sec.	After Letter	.016 sec.	After Letter	.042 sec.
	Range		.464 sec.		.398 sec.		.369 sec.
Sh	Threshold Mean	After Letter	.025 sec.	After Letter	.015 sec.	After Letter	.030 sec.
	Range		.176 sec.		.176 sec.		.166 sec.
St	Threshold Mean	After Letter	.050 sec.	After Letter	.062 sec.	After Letter	.078 sec.
	Range		.140 sec.		.108 sec.		.108 sec.

Under these conditions, whatever the effect of the visual series, if it has any effect, opposite tendencies in direction of displacement ought to be shown in the "H last" from those in the "H first," results, as each is contrasted with "H middle." Contrasted in this way, these results, for A and St, show a relative approach of the mean to zero for "H first," and a relative departure from zero for "H last," or a *decrease of a negative displacement for "H first"* and an *increase of the same for "H last."* In other words, the series draws the displacement of the click toward itself. A *negative displacement is increased by a series coming before the visual stimulus* in question, and *decreased by such a series coming after*. For R and Sh, the negative displacement is increased in both H first and H last as compared with H middle, but relatively the most for H last in both observers. For G there is the same positive displacement introduced by both H first and H last, but it is less than in any of the other cases. The drift of the evidence here, then, is that the *visual series draws the displacement in its own direction*. Each observer who has a negative displacement (Threshold mean after) with "H middle" increases this

when the series all comes before (H last) and two decrease it when the series comes after (H first).

THE EFFECT OF RHYTHM (*Repetition of Auditory and of Both Stimuli*)

It is very evident to any one who has worked at all in the complication experiment, that rhythm plays an important part in the displacement. Witness also the astronomers' experience cited above, St's waiting for the rhythm to establish itself, and my own readjustment to the new conditions when a new combination of intervals was given in the experiment with varying auditory intervals. In order to show the part played by rhythm, I tested each one of five observers on several different days, to fix for each of them both the "click first" and the "click last" thresholds, as above, under each of the following conditions: (1) one visual (single letter) and one auditory stimulus (one pair), (2) one visual (single letter) and many auditory stimuli, and (3) many visual (single letter repeated) and many auditory stimuli (many pairs). For visual fixation, the observer had a very dim light at the end of the observation-tube. The visual stimulus was a flash of red in the place thus fixated. It had a total duration of less than .005 sec. The surface exposed subtended a vertical visual angle of about seven tenths of a degree. In the case of one visual and many auditory stimuli, the visual stimulus was given when the observer had heard the recurring auditory stimuli several times and had himself given the "ready" signal. The results follow in Table VI.

TABLE VI

Obs.		One Pair		One Visual and Many Auditory		Many Pairs	
A	Threshold Mean	After Letter	.005 sec.	After Letter	.022 sec.	After Letter	.042 sec.
	Range		.021 sec.		.024 sec.		.039 sec.
G	Threshold Mean	After Letter	.022 sec.	After Letter	.009 sec.	After Letter	.005 sec.
	Range		.078 sec.		.083 sec.		.084 sec.
H	Threshold Mean	Before Letter	.011 sec.	After Letter	.006 sec.	After Letter	.012 sec.
	Range		.035 sec.		.035 sec.		.039 sec.
Hy	Threshold Mean	After Letter	.034 sec.	After Letter	.030 sec.	After Letter	.046 sec.
	Range		.089 sec.		.074 sec.		.072 sec.
St	Threshold Mean	After Letter	.054 sec.	After Letter	.037 sec.	After Letter	.041 sec.
	Range		.096 sec.		.080 sec.		.083 sec.

In this experiment, the observers A, H, and Hy, show an increasing distance of the threshold mean after the visual stimulus, with the successive introductions of the auditory series and the combined series. In other words, the second column negative displacement is larger than that of the first, and the third column has a still larger. G

and St are again exceptions, as they would be expected to be from the above analysis of their methods. Each did his most accurate work in a case where there was some rhythm present. St said in regard to this work "the one pair abolishes the sound as a standard." The rhythmic factor most missed by these observers, in the case of the single pair, was the sound; for their results are almost the same in the second and third columns. Introduction of the repetition of the visual series does not make any decided difference. A, H, and Hy were able so to adjust their attention as to get the best results in the case of the single pair. The rhythm seemed to introduce for them a subjective rhythm which upset the nice adjustment of attention and so increased the displacement or the time between the threshold mean and the visual stimulus. The negative displacement was increased under these circumstances, probably as a result of the facilitation of the auditory perceptive process. It has an *opened path*. It is a case of pre-perception. Even when both were repeated (many pairs) the auditory dominated, and so did the most at opening its path. But it seems more likely to me that the rhythm, as such, whether auditory or auditory and visual, claimed the attention and so proved a distraction from the work of accurately discriminating the times of the impressions. And this exaggerated the displacement or lack of discrimination in whichever direction it was tending before.

In the successive stages of the investigation thus far, the complication experiment has been stripped down by degrees to the simple problem of the shortest possible interval between two disparate stimuli, — in this case shortest auditory-visual and visual-auditory intervals, as in the one-letter experiment of Table IV and the one-pair experiment of Table VI. The various factors in the complication experiment which have been successively analyzed out — the interval between members of the auditory series, the length of the visual series, the position of the visual series in relation to the auditory stimulus, and the auditory series itself — have all been shown to be factors intimately connected with the way the observer attends to the stimuli in question. From the present standpoint, it may be said they are all factors which, being introduced into the simple interval discrimination experiment, modify the resulting judgment with regard to the interval, by an interference with the normal attention-processes in the discrimination of intervals.

Interval Discrimination

The method of interval discrimination deserves special consideration. Some of the introspective observations made by observers while engaged in the work, already reported, are instructive in this connection. In the case of a single pair, one observer said, "I know which is first because it gets hit first." This remark is a very apt expression of my own experience in trying to answer the same question. "Getting hit first" clearly means, to my mind, some kind of *action* on the part of the observer. He was ready, in the moment of preparation for the experiment, to see a flash of red with his right eye (either eye could have been used) and to hear a click with his left ear. (The stimuli were each produced 25 cm. from the respective sense-organs.) His preparation consisted in securing the "hair-trigger" condition in the two parts of the cortex and conduction apparatus immediately in question in the sensing of the two expected stimuli, and other parts are in a shut-off-from-discharge condition. This is the interpretation which seems to me an appropriate explanation of the feeling of special readiness to discharge in these two directions, when the expected stimuli shall come. The eye- and ear-muscles, in such case, are held tense on the sides (in the organs) where the stimuli are expected. The breath is held, and the whole trunk is under a strain. All bodily processes, in so far as they are controlled, are directed in such wise as to get whichever of these expected impressions shall come first, in as short time as possible, in order to know that it is first.

The reaction which gives the basis for the judgment may be a conscious "hitting" of the first. Or it may be a reaction, ostensibly as a part of the whole apperceptive process of which the auditory and visual processes are parts. This reaction may be any one of many kinds. Often it is a letting-go of the held breath. The exhalation or other reaction comes in response to the whole stimulating or "setting-off" process, and the one or the other of the two stimuli is judged to be first by certain peculiar relations within the experience of the moment. Such an explanation is in part suggested by the expression of St, that the visual impression when it came before the auditory, appeared as a "grace-note," and when it came after the auditory, as an "after-strike." St played the piano. He himself thought that this discrimination was a motor affair, *i. e.*, a difference judged on the basis of a difference in the motor response. The judgment of the temporal order of the two impressions seemed

to be an interpretation or translation of the different motor responses.

A, whose method brought the shortest range in Tables IV, V, and VI, said, "I hold my breath at the moment of expected stimulation, and it goes at the first impression." At another time he said, "When I say 'click first' I have the feeling that the click is left, and when I say 'click last,' that the click is on the right." He interpreted this to mean that when the click sounded first, he had moved slightly toward it, that is, to the left, and that when the visual stimulus had come first, he had moved slightly toward it (it was sensed by his left eye), and this was rather away from the sound, which would have come before the movement could have been more than initiated.

In my own case, I felt distinctly different motor responses in the two cases. There was an immediate feeling of release in whichever organ the stimulus first reached. A little involuntary jerk occurred in the musculature of this sense-organ, and sometimes the head moved slightly in the direction of the first stimulus. The condition of the next moment from which the judgment proceeded seemed to be best expressed thus, "I had it at a time when the other was not there." The attention was accurately set for both. Right eye and left ear were both distinctly innervated. The first stimulus "struck" the appropriate organ, and the "set" of the organ was released.

I am persuaded that the difference in sensitivity to intervals between auditory and visual impressions is due, in part, to a difference in the power of "cocking the ear" to hear, as one fixates the eye to see. The observers who got the smallest ranges between upper and lower thresholds had the most distinct kinæsthetic sensations in the moment of preparation, in the middle ear and about the external meatus. All had some sensations from the side of the head in question. The less accurate had a general feeling in the neck-muscles. Accuracy of discrimination was in no wise connected with voluntary control of the musculature moving the pinna. This was subject of careful enquiry with all observers.

If this introspective evidence leads me aright, it seems that the non-discriminable interval between auditory and visual impressions is due principally to two things, (1) the impossibility of perfect balance in the preparation of the attention for two expected stimuli, and (2) the possible difference in time it takes to react to the different impressions. The various complicating conditions which are added to the simple interval discrimination in the cases of a complication experiment, such as we started with in this investigation,

are chiefly interferences with the first-named factor. They disturb the nice balances of attention. In this simple discrimination experiment, under favorable conditions, a close approximation to a balance can be attained. Any difference in the reaction-times to different stimuli will remain as a constant error of displacement. It is well known that reaction-times to visual stimuli are longer than those to auditory. There is a retinal inertia which delays the perception of the visual impression, in comparison with the auditory, coming from exactly simultaneous stimuli. Having this physiological basis, it will be relatively constant, as compared with the ever-varying attention-differences.

The Coexistence of Mental Processes

Having given, then, these relatively fixed temperamental conditions of reactions to different stimuli, which remain after practice (training in the control of attention) has reduced the reactions to their lowest terms, and has secured the conditions which are favorable for the best balancing of the attention, there is yet one other question very germane to the subject. It will have occurred already to any one reading the above, that while the response to one stimulus is being made, the other may be held in abeyance in the fringe region of the attention-field, and that it is only brought up to clear perception when the first has been disposed of. In other words, it may well be that the first of two simultaneous but disparate stimuli, which gets a start at setting-off its appropriate response in its sense-organ, will bring out this response and be perceived before the other one gets started, — that we do only one thing at a time, — that even in such minute processes as this there is no possibility of division of attention. It is hardly probable on the basis of the experimentation already reported, that this is the case. There is some division of attention. Otherwise there would be an equal certainty of judgment in every case, no matter how small the separating interval. But still the question as to how two mental processes, starting at the same moment of time, do proceed, as compared to the progress of each of the same processes when it holds the field alone, is very vital to the understanding of the psychology of interval-discrimination. And thus the question of objective time-relations is necessarily involved in that of making judgments of the time-relations of simple mental processes (subjective time-relations). The question is, Do these processes, starting simultaneously, proceed just as freely as if they were the sole occupants of the field of attention and so had the whole energy

of attention concentrated upon the single process, or do they *interfere* with each other?

Distribution of attention, of some sort, is granted. It is generally conceded that there must be some sort of overlapping of the processes in any complex mental operation. But there is the greatest lack of information as to how this overlapping takes place, — as to the mechanism of the distribution of attention. Fechner held to the notion of a fixed maximum of available psychophysical energy. If this energy is being consumed in a single process, that process is very vivid, and all other processes are below the threshold. If, on the other hand, it is distributed over several simultaneous processes, they are all of diminished vividness. Distribution of attention always means diminution of vividness, and concentration of attention, increase of vividness. (See Elemente der Psychophysik, vol. 2, p. 451, 1860.) There is no question of the truth of the last statement, and very likely Fechner's fundamental concept is a true one; but there is need of more definite data on the conditions and nature of simple mental processes occurring at the same time, before it is considered proved.

Such researches as those of Paulhan,[1] Jastrow[2] Loeb,[3] and De Sanctis[4] all dealt with the combination of processes which were themselves quite complex. It may well be that such processes as reciting a poem, performing a subtraction or multiplication of long numbers on paper, or keeping time with a metronome with the hand, seem to go along together when combined, so that the time taken to do the two of them together is much less than the sum of the times required for their separate performance, and, in some cases, no greater than the time required for either alone, and yet there may be no real proceeding together. The apparent saving of time may be due, as Paulhan suggested, to a rapid oscillation from one to the other of the two complex processes which are largely automatic and can proceed, to such extent as they are automatic, without any attention. This illustrates how these investigations have probably missed the real point at issue with regard to the division and distribution of attention. The attention might be distributed over several of the minuter part-processes of these processes so that many were proceeding at the same time, and yet the method of these experiments

[1] Paulhan: Revue Scientifique, vol. 39, p. 684.
[2] Jastrow: American Journal of Psychology, vol. 5, p. 239.
[3] Loeb: Archiv. f. gesammte Physiol., vol. 39, 1886.
[4] De Sanctis: Zeitschrift f. Psy. u. Physiol. d. Sinnesorg., vol. 17, p. 205.

would not reveal it. They were not planned with sufficient precision. There is a problem in the division of attention which they did not come within sight of, and this is the real question of division in case of the simplest processes.

This problem is really that of the mechanism of mental assimilation. The process it investigates is illustrated by the maturing collective idea, as a melody or a spoken sentence. There is a gradual enrichment or growth in meaning, as such a process goes on toward its completion. At any instant during the process, implicit associative and nascent perceptive elements are working together to their own mutual clarification and explication. All focal content is the result of complicated interworkings of such fringe material. It is impossible, it seems to me, to question the causal relation of the fringe elements or processes of one moment to the focal of the next; and it is equally impossible to deny the complication of these same fringe processes. They must go on at the same time in order to enter into one and the same resultant process. The question of direct interest at this point in the discussion is, To what extent do they proceed at the same time?

It would seem from the way in which this question, of the relationship and interference of mental processes which proceed or start to proceed at the same time, has come up in this investigation, that the natural method of pursuing it would be that of comparing reaction times for cognition reactions to the single and combined stimuli. But we are warned against this by very clear inferences from an investigation of Professor Münsterberg's in which he used the reaction method.[1] By an ingenious use of the reaction experiment, the author shows that two-part processes in a reaction, as, for example, a restricted judgment of class and a subjective preference, occupy about the same time when combined in a single reaction as when either is performed in a separate reaction. In other words, two judgments of distinctly different kinds can be made in the same time as either can be made when it has all the attention concentrated upon it. The conclusion that these elementary processes go on together — that at least there is some degree of overlapping — seems unavoidable.

But when the first part of the same report is considered in relation to the second, it is clearly shown that the reaction experiment is not adapted to the finer investigation of this problem. For the first part

[1] Münsterberg: Willkürliche und unwillkürliche Vorstellungsverbindung, Beiträge zur experimentellen Psychologie, vol. 1, pp. 64–188.

shows that no matter how much a *motor* reaction is complicated by choices or other judgments, it always takes place in just about the same time as the simple reaction. The complications may be such as actually to double the reaction time in the case of a sensory reaction, and yet a motor reaction, under precisely the same conditions as far as they may be the same for a motor, shows no increase in time. The "set" of the attention in the motor reaction is, no doubt, such a change in the order of succession of the parts of the process that some of those, which come after the stimulus is received in the case of the sensory reaction, are made to come before the stimulus in the motor. When, however, it is found that the motor response to the question, "Is this the name of a scientist, philosopher, poet, statesman, or musician, — Sappho?" is made by the appropriate finger, as previously agreed upon, in just as short a time as the observer can make a motor response with any finger to a simple auditory stimulus, it indicates, either that the whole of the choice judgment has been made before the stimulus was received, or that the judgment itself is so automatic that it is practically a reflex. This latter cannot be true. The judgment, as conscious choice, cannot be made before the stimulus is given, *i. e.*, until the question is completed. And judgment cannot be made automatic and yet be a judgment. In fact both alternatives are untenable, and there is no other course than to hold the situation which gave rise to them at fault. If the judgment process here required previous to the reaction does take time apart from the processes of the simple reaction, the reaction process is shown by these experiments to be unable to exhibit it. A more microscopic method is demanded before the matter can be settled.

The Leipsic method of measuring the scope of attention by means of the tachistoscope is the standard means of securing data as to the *number* of elementary processes which can go on at the same time in consciousness. The same question, with which we are here concerned, grows directly out of the investigation of the number of processes which can go on together. Wundt acknowledges the great difficulty which inheres in the investigation of this problem.[1] Cattell's early work with the tachistoscope showing the numbers of letters, syllables, and words, which could be apperceived under the same objective conditions, indicated the great importance of what we may best call *meaning*, in apperception, and its influence on the number of different processes which may proceed together. In fact

[1] Wundt: Physiol. Psy., 5th ed., vol. 3, p. 351.

the number depends upon the definition of the unit with which the investigator starts out. Wirth, the latest emendator of the tachistoscopic method, has shown,[1] in a very thoroughgoing and genuinely constructive criticism of earlier work, that the one question of primary importance in investigations of the content of the moment of consciousness, *i. e.*, scope of attention, is to set forth the relative clearnesses of the elementary processes there proceeding together. He shows that the different grades of clearness which may present themselves in the field of consciousness of a momentary act indicate, on the one hand, the impossibility of sharply distinguishing the "scope of attention" from the "scope of consciousness" as Wundt uses these terms, and, on the other, the serious indefiniteness of any merely numerical statement of the scope of attention. His main purpose is to set forth a method by which this field can be enriched by exhaustive statements of the relative clearnesses of the processes going on at the same time. All the work of the present study had been performed before the publication of Wirth's work. Otherwise some of his suggestions would have been used in the plan of the experiments following. I may say, however, that I believe the method here used has its own distinctive merits.

General Method for Tests in Coexistence

Taking the suggestions offered by Professor Münsterberg's study of apperceptive and associative processes, I selected simple *judgments of comparison* as the best means of trying-out this question of coexistence. The perceptive act itself is made up of judgments, and these may very properly be the processes studied in combination as in the tachistoscopic experiment. But the judgment which has a previous perceptive act as its condition, determining its start, seems to be better under control. It is itself a central process, not dependent upon the variations of the objective factors in sensation. My plan was to have the stimuli so arranged as to give rise to two or more perceived conditions at the same moment, and so have one or more judgments of comparison between the perceived features made at the moment the perceptions were completed and immediately stated. If one makes two series of single judgments of comparison, and a series wherein these two judgments are combined in a single act, all three under precisely the same objective conditions, and the same subjective conditions, saving only the necessary changes

[1] Wirth: Zur Theorie des Bewusstseinsumfanges und seiner Messung, Philos. Studien, vol. 20, p. 487, 1902.

in the *direction* of the attention, and the percentage of correct judgments is recorded in each case, providing always that in no single series of judgments were the conditions such that all judgments could be correctly given, he would then have reasonable grounds for making inferences with respect to the *interference* of simple mental processes going on at the same time, — whether there is any, and, if there is, how much there is. *Interference* would be indicated by the *falling-off in percentage of correct judgments as the combinations were increased.*

Such relative accuracy of judgments, single and combined, was the test sought after and relied upon in the following experiments. It was very necessary to have the objective conditions such that the results in cases of single judgments, later to be combined, should be short of absolute correctness, in order that interference from the combination should show itself in impaired accuracy. Otherwise there might be some *free energy of attention*, which could readily take up the extra work when the judgments were combined, and so there would be no impairment of accuracy. It was the aim to have the objective conditions, such as duration and extent, so regulated that about ninety per cent correct judgments resulted in the series of single judgments. If, then, when two were combined, eighty per cent were given correctly, and when three were combined, seventy per cent, the inference would seem reasonable that this falling-off in correctness was due to interference. The failure of the perceptive process, indicated by the ten per cent incorrect judgments in the series of singles, would remain a *constant* source of error throughout.

There is, however, one other source of increasing error, with the increasing combination of judgments, supposed above. The judgment processes might go on at the same time without any impairment of the accuracy of the single judgment, and yet the results, as expressed, might show a falling-off in accuracy. This imperfection would then be due to a partial failure of the retentive and reproductive processes, and not to the imperfection of the judgment processes. I have found no sure means of separating this factor and excluding it. As the experiments were arranged and conducted, though, I believe any impairment in accuracy resulting from combination is more likely due to interference of the judgment processes.

If neither of these factors is efficient, on the other hand, there will result no falling-off in accuracy of results when single judgments are combined. To be sure the conditions of the experiment, as outlined so far, do not preclude the possibility of the combined judg-

ments occurring in succession, and so giving rise to as large a percentage of correct results as when occurring singly. That is, while one of the so-called combined judgments was in process, the latent conditions of the other would remain for the moment as mere physiological or possibly psychical dispositions, and to these one would "hark back" in the next moment. Here the reaction method is suggested as the means of assurance that this is not the case. But this method, we have already seen, will not lend itself to work of such precision as this. The probability of this succession of judgments is reduced to a minimum in the experimental groups following.

It can be practically precluded by a prevention of all sensory images. In these experiments every precaution was used to prevent them. In all cases where visual stimuli were used, for instance, a brightly illuminated blue field immediately succeeded the momentary stimulus, while comparative darkness preceded it. I cannot be so sure that there were no memory images functioning. But all observers were carefully questioned on this point at frequent intervals during the experiments, and no evidence of their existence was found in any case. I feel sure sensory images were excluded and think memory images very improbable.

Single and Combined Judgments from Visual and Tactual Stimuli

In this group of experiments, I used judgments from visual and tactual stimuli, singly and in combination. Both stimuli were given by means of a large pendulum in the Harvard laboratory, specially constructed for Professor Münsterberg. This pendulum is about one and a half metres in length. It is hung in a heavy steel frame which rests upon a large table. A curved steel bar, concentric with the swing of the pendulum, and ninety degrees in extent, is so set to the frame that it serves as the attachment for an electro-magnet, at any point in the swing of the pendulum. The pendulum-rod carries an armature which fits this magnet. By means of this magnet, the pendulum may be held at any point between the position of rest and forty-five degrees out in either direction; and it may be released by *breaking* the circuit through the magnet. The pendulum also carries a segmental screen of about seventy degrees extent. An opening about nine by eight centimetres near the centre of the screen affords means of tachistoscopic observations. A sliding shutter makes the slit as narrow as may be desired. In these experiments a black tube was set up, at right angles to the direction of the motion of the pend-

ulum, and at the height of the slit in the screen. On the other side of the pendulum screen, and directly opposite the tube, was placed a support for holding the object to be shown.

The object for the visual stimulus was one of two light gray lines on a black background. These lines were 4 mm. wide, and one 44 mm. long, and the other 40 mm. The work was done in a dark room. The stimulus card was illuminated by an electric light hanging between it and the screen. Both cards were shown the observer several times, before experimenting, till he was sure of their lengths. Upon one being shown, in experiment, he was asked to say whether it was the longer or shorter. The touch apparatus was so arranged that the experimenter could at will give the observer one or two contacts on the back of his right hand. The contacts were made by means of an electro-magnet. This was actuated by a current which was *made* by the closing of a switch which was secured by a set-screw to the same curved steel bar as bore the pendulum magnet. This switch was closed by the pendulum in passing. It was adjustable on the bar. Another similar switch, opened by the falling pendulum the next instant, removed the tactual stimuli. These switches were so placed in the course of the pendulum fall that the tactual and visual stimuli were exactly simultaneous. The tactual judgments were, *one* or *two* points touched. Results are presented in Table VII for three observers, A, B, and Bo. The number of series which were averaged in each case is given, in order properly to weight the results.

TABLE VII

Obs.		*Single Tactual*	*Single Visual*	*Combined Tactual and Visual*
A	Number of series averaged	3	3	3
	Per cent Correct Judgments	80	89	79
	Average	84		
B	Number of series averaged	5	5	5
	Per cent Correct Judgments	72	78	78
	Average	75		
Bo	Number of series averaged	5	5	5
	Per cent Correct Judgments	88	71	78
	Average	79		

In Table VIII are given A's results for further experimentation under the same conditions, also for a pair of visual judgments, a pair of tactual, and all four combined. One series of each of the five was given each hour of experimentation. For the additional visual judgment, the observer was required to say whether the line was *high* or *low*. It was of two heights from the lower edge of the card, 17 mm. and 20 mm. For the other tactual judgment, he reported the point or points touched on the hand, as on the *right* or *left* side. The middle line was traced by the experimenter, before tests, as often as the observer wished to be reassured of its position.

TABLE VIII

Obs.	*Sing. Tact.*	*Sing. Vis.*	*Tact. and Vis.*	*Two Tact.*	*Two Vis.*	*Two Tact. and Two Vis.*
A Number of series averaged	6	6	6	6	6	6
Per cent Correct Judgments	87	88	83	90	81	83

The next additional combination was a pair of judgments based upon auditory stimuli. Four electric clickers were placed on the wall behind the observer. Two were loud and two faint. Each pair was accurately adjusted so they were of the same intensity and quality. One of each pair, *i. e.*, one loud and one faint, were hung about four feet to the left of the observer's median plane. The other two were hung at an equal distance to the right of this plane. The circuit making the click was *made* by a switch closed by the pendulum as it fell. The experimenter by pressing any one of four buttons gave the one of the clicks he desired. The observer's two judgments were as to the *loudness* and the *position of the click.*

TABLE IX

							Six Judgments Together					
	Two Vis.		*Two Tact.*		*Two Aud.*		*Visual*		*Tactual*		*Auditory*	
Obs.	*Lgth.*	*Pos.*	*Num.*	*Pos.*	*Inten.*	*Pos.*	*Lgth.*	*Pos.*	*Num.*	*Pos.*	*Inten.*	*Pos.*
B Number of series averaged	8	8	8	8	8	8	14	14	14	14	14	14
Per cent Correct Judgments	82	94	89	100	94	92	89	97	86	96	70	86
Average			91.7						87.3			
Bo Number of series averaged	12	12	12	12	12	12	20	20	20	20	20	20
Per cent Correct Judgments	77	91	87	97	81	82	77	93	85	98	81	80
Average			85.8						85.7			

It is evident, on the face of these returns, that there is no positive assurance of interference. Each of these observers had been in some part of the complication work. And so the inference from lack of evidence here can be carried back to that work, and we may rest assured that the lack of accuracy in interval discrimination work by these observers was due in minimal measure, if in any, to interference of the mental processes, auditory and visual, tending to proceed at the same time. Some parts of the results here presented look like evidence for interference. But there is, on the whole, just as much evidence of what one might call facilitation, in combination, as there is of interference.

There is one source of possible explanation for the non-appearance of evidence of interference in these results: that is the fact that the stimuli are disparate, and so probably take different times for maturing. Thus the judgment processes, so far as they thus start from disparate sensations, may start at different times. There was good reason for using disparate stimuli first for the combination of two mental processes, as this was the closest related to the simple interval discrimination experiment to which the complication experiment had been reduced. But this objection is now easily overridden by making the conditions of experiment such that all judgments start from one and the same perceptual process.

One, Two, and Three Judgments based upon a Single Sense-Perception

The conditions here were such that the perceptual basis for any one of the single judgments was at the same time the possible basis for any other single judgment and also for any or all of them combined. What judgment or judgments were given depended entirely upon the directions given, and the consequent preparation of the attention. Under these conditions, there could no longer be any doubt about the even start of all judgments, so far as outer conditions were concerned. The only remaining cause of an uneven finish — lagging of a process, as shown by its increased inaccuracy when combined — must be interference with its progress by other processes going on at the same time.

Visual stimuli were used. The objects to give the perceptual basis for the judgments were small rectangular openings in cardboard seen, on exposure, by *transmitted* light. These rectangular windows in the cardboard were 2 cm. by 1 cm. and stood in the vertical position 1 cm. apart. The judgments were all based upon differences exist-

ing between these rectangles as shown. One of these differences was in *length*. They might be of the same length, or either the right or left might be 2 mm. longer than the other. Another difference was in *shade*. This was secured by different thicknesses of paper, pasted over the openings. Two shades were used. The opening on one side might be shown as either the same brightness, brighter, or less bright. The third difference was in the *number of lines* which crossed the rectangles. Two or three wires were placed across them horizontally and about 5 mm. apart. Thus they had the same number of lines, or one had fewer or more than the other.

The same large pendulum was used in these experiments. The moveable magnet on the curved steel bar was kept in one position throughout. It held the pendulum, ready for release, at twenty degrees from the position of rest. The adjustable weight on the pendulum was also kept in one position. The only adjustment which was changed during this series of experiments was the width of the slit in the window of the screen. This was varied from one millimetre to five. The whole time during which any part of the two rectangles was in view (the total exposure) with a 5 mm. slit was .033 sec.; with a 3 mm. slit .031 sec.; with a 1 mm. slit .029 sec. These times were measured with a Hipp's chronoscope. The entire visual field, embracing the two rectangles, was about 2 cm. by 3 cm., and was about three fourths of a metre from the observer's eye. It could be accurately fixated beforehand and fully exploited during the moment of exposure.

The observer was always instructed to give his judgments in terms of one of the two rectangles. If, for example, length was in question, he should say of the left-hand rectangle that it was longer, shorter, or of the same length as the right-hand one. The process of expressing the judgments was also facilitated by using the terms plus, minus, and equal, for all three sorts of judgments. This was a special aid to expression where two or more judgments were in question at the same time. In these cases the observer was always given an order beforehand, in which the judgments were to be given. This order for the three combined, for example, was always, "length, lines, shade," as in the following tables. If, then, the judgments were given "plus, minus, minus," it meant that the left-hand rectangle was longer, had fewer lines, and was less bright than the right. The process of making these interpretations, as well as the order, was made automatic with the observer, by practice, before experimenting.

Three observers, A, B, and Y, were used in this experiment. The

judgments were made in series of ten. Each hour's work was distributed over (1) several series of single judgments, (2) two at a time, and (3) three at a time, the aim being to get an equal number of judgments of each kind, length, lines, and shade, under each of the three conditions. The results are given as general percentages of correct results. To properly weight these averages, the number of series (of ten judgments each) which are included in making up any average, is given just above the average.

TABLE X

	Single Judgment			Two Judgments			Three Judgments		
Obs.	*Length*	*Lines*	*Shade*	*Length*	*Lines*	*Shade*	*Length*	*Lines*	*Shade*
A Number of series averaged	11	12	10	15	17	14	11	11	11
Per cent Correct Judgments	95	93	96	90	91	83	94	91	89
Average		95			88			91	
B Number of series averaged	8	8	7	9	10	9	7	7	7
Per cent Correct Judgments	93	80	90	76	77	90	75	70	75
Average		88			81			73	
Y Number of series averaged	12	13	13	19	19	16	13	13	13
Per cent Correct Judgments	76	80	58	72	76	61	72	74	58
Average		71			70			68	

Since these general averages for the single judgments are so close to those in pairs, it seemed possible that the presence of objective differences, other than the single one asked for, might be a distracting agent, and really interfere with the judgment process in question. For example, when judgment on length was in question, it might be possible to give it correctly a larger number of times, if there were no differences in shade or lines, than if these were present. Some careful test experiments were made with a view to clearing up this situation. The observers in no case knew the nature of the investigation, nor were they aware that other differences were absent in some of the cases. The results presented in Table XI certainly show that the presence of other differences than the one in question is no cause of interference.

TABLE XI

Obs.	Length With Diffs.	Length Alone	Lines With Diffs.	Lines Alone	Shade With Diffs.	Shade Alone
A Number of series averaged	5	5	5	5	5	5
Per cent Correct Judgments	98	96	96	96	98	94
B Number of series averaged	5	5	5	5	5	5
Per cent Correct Judgments	90	92	96	94	78	84
Y Number of series averaged	7	8	8	8	7	8
Per cent Correct Judgments	73	76	75	64	88	67

Notwithstanding the precautions taken to secure the full energy of attention for the single judgment process, as already indicated in the discussion preliminary to these experiments, — namely, by making the stimulation conditions so near the threshold that only a part of the judgments could be given correctly, — there still appeared a probability that there was free energy of attention during the single judgment process. The observers seemed to do more work when more judgments were asked for. If this is true, the results of Table X are not a true index of interference. If there is free energy during the moment of making the single judgment, this may readily be used for another process when combined with the first, and so there will be no interference. This is a sufficient proof so far as it has immediate bearing upon the interval discrimination experiment, but the further question as to what will take place if we can use this free energy, if it exists, in both processes alike, is an important one for the question of the relation of two processes going on together in consciousness.

To ascertain the fact in this matter, I performed a series of experiments with the same observers, in which previous occupation of the mind served as a distraction. The distraction consisted in a simple arithmetical operation, — addition or subtraction. The moment before giving the stimulus for the judgment processes, — in the place of the "ready" signal, I would call out some numbers, as, for example, "twenty-four from sixty-three" or "fifty-seven and fifteen," the first indicating subtraction and the second addition. The answer to the addition or subtraction was always given before the judgment or judgments, to make sure that it was performed. And in any case where the observer knew that the addition or subtraction was done before he attended to the stimulus for the judg-

ment, that particular test was thrown out. The results are given in the same form as in Table X.

TABLE XII

(*Addition and Subtraction as a Distraction*)

	Single Judgment			Two Judgments			Three Judgments		
Obs.	Length	Lines	Shade	Length	Lines	Shade	Length	Lines	Shade
A Number of series averaged	8	8	8	15	15	16	15	15	15
Per cent Correct Judgments	79	84	67	66	68	66	53	69	59
Average		77			67			60	
B Number of series averaged	6	4	5	8	7	11	7	7	7
Per cent Correct Judgments	57	70	60	66	44	53	51	50	44
Average		62			54			52	
Y Number of series averaged	8	8	7	15	16	15	14	14	14
Per cent Correct Judgments	66	60	51	56	62	56	66	57	55
Average		59			58			59	

These results (general average percentages) show, for observer A, a more regular and somewhat larger falling-off with combination than in Table X, for B and for Y, a diminished falling-off, and relatively less for the three than for the two combined judgments. The percentages are lower throughout. This is a result to be expected. But there is no notable change in the relative lowering of two judgments in comparison with single judgments, or of three in comparison with two, such as should appear if, as supposed, in the experiment resulting in Table X, there had been free energy of attention in the case of the single judgment.

It was my aim in these experiments, with distraction through another simultaneous process, to secure a uniform residue of attention for the judgment processes, whether single, in twos, or in threes. The arithmetical operations were therefore as uniform as possible. But it may readily be that very unequal demands were made upon a given observer by successive operations, one's automatisations in number-work may be so various. These would no doubt tend to average up in the course of the whole work running through several weeks. But in order to make more sure of the point, I tried another means of using the free energy of attention which may exist in the

case of the single judgment, namely, by suggesting a judgment or series of judgments just before an exposure. It will be recalled that the order of judgments was always the same as that of the tables, and that all were expressed as minus, plus, or equal. So if the experimenter called out before a three-judgment exposure, "plus, equal, minus," it would be in the nature of a challenge to the observer to assure himself beyond a doubt whether or not the exposure showed the left-hand rectangle as longer than the right, having the same number of lines, and being less bright. The so-called suggestion was a distinct factor in heightening attention. This is shown especially in Y's case by the larger percentage of correct judgments. Results are averaged in Table XIII.

TABLE XIII

(Attention heightened by Suggested Judgments)

Obs.	*Single Judgments*			*Two Judgments*			*Three Judgments*		
	Length	*Lines*	*Shade*	*Length*	*Lines*	*Shade*	*Length*	*Lines*	*Shade*
A Number of series averaged	2	4	2	6	7	7	8	8	8
Per cent Correct Judgments	90	89	85	87	81	76	92	72	77
Average		87			81			80	
B Number of series averaged	4	3	4	8	9	7	8	8	8
Per cent Correct Judgments	85	70	92	84	80	83	84	74	81
Average		82			82			80	
Y Number of series averaged	6	6	5	11	13	12	16	16	16
Per cent Correct Judgments	88	75	94	90	81	77	89	74	70
Average		86			83			78	

An analysis of the results obtained from B to show the effect of the suggestions is given in Table XIV.

TABLE XIV

	Single Judgments	*Two Judgments*	*Three Judgments*
Per cent of right suggs. judged correctly	87	85	79
Per cent of wrong suggs. judged correctly	73	77	80

The effect of the so-called suggestions in making for correct judgments was then quite noticeable in the case of single judgments, less so in two judgments, and none whatever in three. This observer was

able to overcome 73 % to 80 % of the wrong so-called suggestions. Now, when it is considered that only 80 % to 82 % of all the judgments given by B (see Table XIII) are correct, it is very clear that their action as suggestions was very slight. They had an influence, however. It was shown, as expected, in a heightened attention. This was especially the case with Y. Compare his general averages in Table XIII with those in Table X. This rise in general averages coincides with the impression of the experimenter during the experiment. It seemed then that this was a distinct challenge to keen attention on the part of Y. He is a man who intends to make impartial observations for himself, and has no notion of being told what he is to see. That his general averages of correct judgments stand so much farther apart in this case with heightened attention than in either of the others (see Tables X and XII) is indicative of an interference of the judgment processes themselves.

Such a series of general averages as those of Y in Table XIII, as those of B in Table X, or as those of A in Table XII, seem, in themselves, and under the conditions of the experiment, to be pretty clear indication of an interference of simple mental processes carried on at the same time. The only other explanation is that suggested above, namely, an interference of the processes of reproduction and expression. The conditions of the experiment seem to reduce the probability of this to a minimum. But the centre of interest, in considering the results, does not lie in the question as to whether it is interference of the judgment processes themselves or the processes of their reproduction. The foreground is occupied by a prior question, namely, whether there is any evidence here presented for interference. For if there is interference of such processes, why does it not show up in the results for each of the observers in each of the Tables X, XII, and XIII? Of the nine cases here offered for comparison, only the three above designated show what may be called clear evidence of progressively increasing interference with increase of combined processes proceeding at the same time.

Under these circumstances this cannot be accepted as indisputable evidence of interference. Such results as those of A in Table X, where correct judgments, two at the same time, are given in 88 % of the cases, and three at the same time, in 91 % of the cases, stand directly opposed to interference. They seem to show a facilitation by combination. This is indeed possible where three and only three sorts of judgment are worked with. It is the limiting case, and if more than one is asked for it is really easier to give three than to

select two. A himself remarked that this was the case. A similar explanation holds concerning the results of B in Table XII, and those of A in Table XIII. If such an explanation is the true one, it is manifest that the "limit of attention," of which mention has been made above, has probably not been reached in any of these cases. On the whole, these experiments *seem to indicate a small degree of interference of simple mental processes going on at the same time.* But such interference *cannot be considered proved* by these experiments.

The Question of Synergy

In connection with these last experiments, where the comparative judgments all proceed from one definite perceptive act, and where therefore the conditions are most accurately controlled for showing the effect of interference, if it is a fact, there is yet another means of looking into that question. This is afforded by the *similarity* of the *means of expressing* the different kinds of judgment. In connection with the current emphasis given to the motor side of mental processes, it is often urged that mental processes go on at the same time when they are working together toward one and the same motor out-go. Otherwise they are likely, at least, to hinder each other, and to take their turns. If such is the case, the similarity of motor out-go which is present in these cases, where all three judgments are plus, or all minus, or all equal, ought to produce a larger percentage of correct judgments than is found in cases where there are two or three kinds of expression. Furthermore, if there is no interference of the judgment processes as such, but, as supposed possible above, the impaired accuracy of judgment in the combination of judgments is due to the imperfection of the memory, this too will be diminished by similarity of expression of the three judgments. In fact similarity should reduce this source of error to a minimum. The results presented in Tables X, XII, and XIII, for three combined judgments, were worked over, so far as possible, and all cases where the three judgments, if correctly made, would have been expressed similarly, were separated out. The total number of such cases, and all those where the judgments would have been properly expressed dissimilarly, are recorded for each observer in Table XV. The number actually given correctly under each class is also recorded, as well as the *percentage* of correct judgments in each class for each observer.

TABLE XV

Obs.	Judgments expressed Similarly Total Number	Correct Judgments Number	Per cent	Judgments expressed Dissimilarly Total Number	Correct Judgments Number	Per cent
A	222	172	77	561	479	85
B	195	153	78	498	356	71
Y	372	277	74	863	572	66

If the results of B and Y were presented alone, they would seem to indicate synergy of similarly expressed judgments. But those of A are most strongly contradictory of such a working together of such judgments. This is very surprising to me, as A had such a facility in expressing these similar judgments, especially "equal, equal, equal," that it suggested this comparison. But the apparent facile expression is here shown to have attended a diminished accuracy. No conclusion can be drawn with respect to synergic influence from the similarity of expression of judgments.

RELATION OF OBJECTIVE AND SUBJECTIVE SIMULTANEITY

Reviewing this work in combination of judgments with reference to its bearing upon the complication results, and the interval discrimination results, it seems that interference of simple mental processes going on at the same time, though it appears to be a fact, showing itself in impaired accuracy of processes combined, is yet quite inadequate to explain the whole, or indeed, any considerable part of the synchronism, as we may call the "click first" "click last" interval of Tables IV, V, and VI. The slight amount of interference of such processes as the auditory and visual perceptions, tending to proceed at the same time, would tend to a very slight displacement of one with regard to the other. It is true, for reasons already discussed, that this time-difference is so slight and so difficult of seizure that it cannot be measured, and so no measure is offered. We cannot, therefore, be certain *how much* of the non-detectable interval is due to this cause. But the evidence offered in the above tables of results is ample justification for the statement that *interference* can be responsible for only a *very small part* of the "click first" "click last" interval.

In the case of this interval, as in that of an interval between any disparate stimuli, a part of it must be due to the different resistance or inertia of the sense-organs. The eye is undoubtedly slower than the ear. This would at once suggest itself as the cause of the interval between the threshold mean and the visual stimulus in the results

shown in Tables IV, V, and VI above. That is, vision being slower, an auditory stimulus given at the same time as a visual will appear to be earlier, and it may be given considerably later and yet appear earlier. In general, therefore, so far as this cause is active, one would expect that the interval, at which a sound must precede a visual stimulus in order to be certainly distinguished as coming before the latter, would be much shorter than the interval, at which a sound coming after a visual stimulus could be unfailingly distinguished as coming later. In other words, the centre of gravity of the "click first" "click last" interval, so far as this visual inertia is the cause of its displacement with reference to the visual stimulus time, will be *after* the visual stimulus.

In one case in my results, Table V, St, H middle, there is presented an extreme where not only the centre of gravity (threshold mean) is placed after the visual stimulus (letter), but the whole synchronous period ("click first" "click last" interval) is after the visual stimulus, so that a sound coming .008 sec. after the visual stimulus is distinguished with certainty as coming before it. So also St, in Table VI, one pair, the sound coming .006 sec. after is judged as coming before the visual stimulus.

But the variety of displacements of the threshold mean in different observers, and more particularly in the same observer under different experimental conditions, indicates very clearly that there are factors other than visual inertia which are quite as important, and perhaps equally responsible for this displacement. In Table VI, H, one pair, for example, the threshold mean is before the visual stimulus .011 sec. So in Table V, G, H first, and also H last, it is before the letter .005 sec. In these cases there must be some factor or factors quite as strong as this visual inertia, and counteractive to it. These are, in part, the complex attention factors which have been referred to already. Prominent among them are the rhythmic perception which is so marked in St; the movement toward the first stimulus and the "letting-go" of the breath, of A; the passive "striking" of the letter by the sound, in the case of some of the observers; and the "cocking" of the eye and the ear, of others. These all have to do with the length and place of the "click first" "click last" interval quite as much as does the visual inertia. But however this may be, of this inertia and the other factors just now named, probably each has more to do with it than does the interference of the perception processes themselves.

But after eliminating the parts played by each and all of these

agencies in the determination of the interval, there will remain a period of "present time," in which there are no time-differences, and no qualitative differences which lead the subject to suspect the existence of time-differences. The mental content of this reduced synchronous period in experience is *one experience*. The sound was heard and the letter was seen, but they came *together* as aspects of *one* experience. In the moment of perceiving either one, it was not possible to say that the other was already a memory. In other words, the *primary memory* of either, whichever came first, had lasted over into the perception of the second. There had been no perceivable transformation of the first since the instant of its perception. At the moment of the inception of the second process, the first was still, to the perceiving subject, what it was at the moment of its own inception. Though change was probably going on in the physiological substrata of the mental process in question, in every minutest moment of the interval, yet a certain amount of effect of this change had to accumulate before the observer could become aware of the change, and so be aware of the passing of time or of temporal difference. This was, then, only a case of the working of the law of *relativity*. And the perception of time is a function of the duration and amount of change of mental process.

Looked at from this point of view, we see the whole explanation of the existence, the amount, and the position of this synchronous period under one rubric, if only we could grant the combination of mental processes without interference. If mental processes go on together, the sole ground of the imperceptibility of short periods of time separating mental processes is in the fact that the first of these processes *has not changed sufficiently to be known as different*, to the perceiving subject. The minimal perceivable interval will vary from man to man, and in the same man from time to time, inversely as the amount of change per unit of time, in the process itself. The same statement could be made in terms of vividness or relative clearness. The more focal the idea or process, *i. e.*, the more vivid or relatively clear it is, the more rapid will be the changes and the perception of those changes. Professor Münsterberg's physiological explanation of vividness,[1] as due to the facilitation of the motor discharge, has already found confirmation in the method of keenest interval discrimination as outlined above. The more rapidly the first process can get into action, the more is the discriminated interval shortened. So in Exner's experiments, where it was known which of two stimuli would come

[1] Münsterberg: Grundzüge der Psychologie, vol. 1, p. 525.

first, the interval was very much shorter than any of my results, for the motor preparation could be made very complete beforehand, as in a muscular reaction. Therefore the perceptible change, upon perception of the stimulus, occurred in a shorter time. Under any circumstances, the conditions, subjective or objective, which make for rapid maturing (and by the principle of dynamogenesis maturing means going over into action) of the mental process, make also for the shortening of the least perceptible interval.

These conditions are as various as the gamut of human experience is wide. There is nothing, from the primary temperamental characteristics to the passing wave of feeling of the present moment, which does not affect it. Most particularly, though, is it a matter of the relations existing among the elementary processes striving to go on together. Among the focal and fringe elements of a given moment of experience, no matter how carefully the practised introspectionist may strive after an ideal condition of monoideism, there is an incessant interaction. There are all sorts of hindrances and facilitations. Herein is the justification of Stern's statement that the "praesenzzeit," as he calls it, "varies with the quantity and quality of conscious content, the direction of attention, and the strength of psychical energy," and that it cannot be assigned a maximal value but rather what he calls an "optimal value." All that is included, in fact, in the complex rubrics, *attention* and *interest,* has to do with the length of this indiscriminable interval.

Time-difference in consciousness is the very simplest thing in mental life, for it is a case of the bare awareness of change. The elementary time-judgment is mere judgment of change in content of consciousness. In the experiment where one is asked to say which of two expected stimuli comes first, however, the case is already complicated. There must be a double preparation to react and to note the change characteristic of each case, and so convert it into a time-judgment. In the combination of two judgments, there is the same double expectancy, preparation to react in two ways at once. In each experiment, the preparation and shaping of expectation is the same as in reaction experiments. In all reaction work, the short reaction comes as the result of catching the attention wave at its most favorable point. If the signal to react catches the idea of reaction in the mind of the observer at the very focal point in consciousness, the shortest reaction possible under the given conditions results. So in both the combination experiment and the interval discrimination experiment, it is very necessary to catch the attention wave, *equally prepared for*

both or all the processes, and at the highest crest of advancement. Both demand the same preparation as a compound reaction. I believe it is this inequality of balance of the attention between the various processes that is responsible for the interference which is evidenced in my results. This is my explanation of the appearance of impaired accuracy for combinations for a given observer under some conditions and the failure of any sign of impaired accuracy for the same observer under other experimental conditions, or even under the same experimental conditions at different times.

In the time-interval discrimination experiment the evenness of balance in the attention wave will make for the shortest interval discrimination, and the proportion between the two will be direct, so far as other factors do not interfere. But there are special interferences here. One of these is the fact that the two mental processes do not set off at the same moment. No matter how even the balance in attention at the moment of impact of the first of the two stimuli, the preparation for the other, not yet set off, cannot be held in equal readiness while this is going off. This discharge has already disturbed the preparation to discharge in the other direction. In the case of a given pair of stimuli of definite qualities and intensities, the relation will be one of mutual facilitation for one interval of separation and one of inhibition for another interval. In one case the first opens the path for the second, being a case similar to the summation of stimuli, and in the other, it draws all the available energy in its own direction.

THE ESTIMATION OF NUMBER

BY C. T. BURNETT

I. THERE are situations not a few in life in which we find ourselves estimating the number of objects in some group. Sometimes we desire to know merely whether the group is large or small. Sometimes we try to reach an absolute number that shall approximate roughly to the real number. Sometimes, again, we only care to know whether the group in question is more or less numerous than some other group that we have before us or perhaps recall in memory. The public speaker finds himself wondering whether this present scattering audience is larger than the one that last night crowded into the front seats. The farmer riding between adjoining orchards judges roughly the prospective yield by a comparative estimate of the fruit in sight. The politician too has an interest that is very notable indeed in such rough numerical estimates. He asks himself, for example, whether the voters will be more influenced by reports favorable to his party sent in from numerous small towns or by such reports from a few large centres. Or perhaps he is planning a demonstration in favor of his candidate. His problem then is so to arrange his procession that five hundred men will look like five thousand. Turning to another field, how is it that the enrolment in some institutions of learning seems larger and the size of the faculty more portentous than in other similar institutions that are really of about the same size?

These examples bring to mind our interest in rough numerical estimates and at the same time suggest the probability that we are swayed back and forth in these estimations without ever a numerical difference occurring in the objects of our judgment. These considerations lead us on, then, to an enquiry about the factors that can thus influence our estimation of number.

II. Influence of Factors in the Same Sense-Field as the Objects whose Relative Numerousness is in Question.

The experiments described in the following pages are concerned with the influence exerted on the judgment of a given factor by other

factors presented at the same time. The object of judgment in these studies is visual number, which is to be submitted under varying conditions of the objects whose number is in question, for example, varying conditions of form, size, distribution, with the intent to discover whether this judgment is a function of these other factors as well as of the numerical. The scope of the enquiry includes both relative and absolute number.

The objects chosen as a basis for the number-judgment were bits of paper pasted in two well-defined groups side by side upon a background of black cardboard. This card fitted into an upright frame where it was held in place by a pivoted spring, which allowed easy adjustment and removal of the card. The opening of the frame, 15×20 cm. was concealed at will from the observer by a black wooden screen that played up and down on guiding posts, when released by a cord and lever from the catch that held it in place before the card. It fell by gravity upon a cushion that deadened the sound; and it was restored to its position by the operator's thrusting his fingers beneath and lifting it till the catch above caught and held. The entire apparatus, as well as the operator's movements, was concealed from the observer by a large black cardboard screen resting upon a black-covered table. The one opening in this screen was just large enough to allow a full view of the card when the inner wooden screen fell from sight.

This apparatus which we will call the Two-Group Apparatus, admitted of simultaneous exposure of the two groups of objects, and that only. At first, to make successive exposure possible, a light wooden frame was constructed in whose grooves two leaves of black cardboard ran like sliding doors. By means of rods fastened to their outer edges these leaves could be pulled apart or thrust together till their inner edges met. When this apparatus was placed between the outer screen and the frame bearing the card, and the inner wooden screen had been dropped out of the way, this substitute divided screen was sufficient roughly to accomplish the end in view.

With this apparatus the illumination was daylight, coming through a very large window at the back of the observers. By means of a curtain, marked variations in the light could be prevented.

For the length of simultaneous exposure of the groups the following rule was adopted: Each observer was to be allowed time enough to get a satisfactory feeling of relative number, but not time enough to admit of counting. This time was kept constant during the work of any one sitting. As the weeks went on, it was found possible,

under the rule laid down above, to shorten the time for some of the observers, and to use with all the same length of exposure that had sufficed for the speediest. The range of variation was from 1.2 sec. to 1.6 sec. Time was measured by the ticks of a watch. Later tests showed for the time studied that, where effective at all, the longer exposure diminished a given tendency. Often it had no apparent effect.

The method of control already described is not only rather rough but does not exclude the possibility of a space error. This possibility proved actual by experiment. So an apparatus was contrived that should present the groups in succession at approximately the same place and should shorten the exposure, if desirable, to a small fraction of a second.

This new apparatus, which we will call the One-Group Apparatus, required artificial light and a dark room. By means of a 125 cp. incandescent electric lamp, images of the groups of objects were reflected through the lens of a camera and came to a focus upon its ground-glass screen. A second screen of ground glass was placed in front of the first and as close to it as possible, that an even distribution of light might be obtained. The cards containing the objects were of the same general character as in the earlier experiments. They were held in a moveable slide whereby each group in succession could be brought before the lens. When the slide was drawn to the limit in one direction a single circle appeared in a black field. This circle was used as a signal and a means for directing the eye in the dark to that region where the groups were to appear. The exposures were made with a camera bulb, the shutter being set for instantaneous movement, with diaphragm 22 and length of exposure $\frac{1}{25}$ sec. A shorter time was thought on trial to make perception too difficult. The apparatus rested upon a table of special construction and was enclosed as far as the glass screen with a wooden frame covered with denim. Double curtains of this material formed this enclosure on one side and made possible an easy adjustment of the cards between exposures, as well as the admission of the operator's hand during a given experiment for the adjustment of the shutter. This had to be set, of course, before each of the three exposures constituting one experiment. During its progress the hand was not removed at all, the curtains falling about the arm in such a way that little light escaped. The other hand managed the moveable slide from behind the enclosure.

Time was measured by watch-ticks. The three exposures — dot-

signal, Group 1, Group 2 — were separated from each other by intervals of 1.6 sec. This was fixed upon as the minimum for convenient operation of the apparatus.

In much of the experimentation on relative number two observers were employed at once. Their chairs were placed closely side by side on a line about 150 cm. from the plane in which the groups appeared. These groups were not very far from being on a level with the eye. Each observer recorded his own judgment, against the number of that experiment. There were three possible kinds of judgments, — equality or either group larger. If the judgment was of difference it was recorded in terms of the larger.

When the dark room was used, special arrangements were required, for convenience of the observers in making their record. After several schemes were tested the following was adopted as least trying to their eyes: A large, black-topped table was placed before them, bearing an electric lamp enclosed in a black box with a small aperture that could be closed at pleasure; or, if left open, did not let enough light escape to disturb the perception of the groups.

The absolute number of objects in the groups was determined, first, by the character of the problem, and then by convenience. If we are to learn anything about the influence exerted upon the number-judgment by other factors than the numerical, we must eliminate all influence of the latter. Correct judgments may be determined by this factor alone; erroneous judgments must have been otherwise conditioned; and these conditions it is the task of our method to isolate and study, as modifying factors. From correct judgments we learn nothing definite about our problem, but from erroneous everything. Other things being equal, it is preferable to eliminate from the results the influence of this numerical factor, just as one handles any other disturbing, unavoidable element, by equalizing the numbers in the two groups.

What may be called the standard number of objects in each is twenty. This choice was governed by the purpose of using a number large enough to make counting impossible in a brief time and yet not so large as unnecessarily to increase the labor of preparation and the difficulty, for the observer, of getting an idea of the groups as a whole. To the cards containing equal groups, 20 to 20, were added others, 20 to 19, 19 to 20, for the purpose of easy variation in arrangement, by omitting one object from a group, without making the actual numerical difference easily perceivable. In later work these small objective differences were dropped. Yet other

cards, 23 to 17, 17 to 23, were added, to the end that the observers might find unmistakeable number-differences, and so not be bothered by the suspicion that the groups were all equal. The reversal of the number-relations, as indicated above, was in the interest of equalizing the influence of the actual numerical factor in the two groups.

The following proportion was kept among the numbers of observations made upon each kind of card: $\frac{1}{2}$ upon groups objectively equal; $\frac{5}{12}$ upon those differing by one from each other, where half each went to (20 to 19) and (19 to 20); $\frac{1}{12}$ to those showing the maximum objective difference of six, where again half went to (17 to 23) and half to (23 to 17). Of course the observations upon cards of this last sort are excluded from the tables.

As to the number of cards employed for each series of experiments, it was found at first convenient to use seven,—3 (20 to 20), 1 (20 to 19), 1 (19 to 20), 1 (17 to 23), 1 (23 to 17). In each group the arrangement of objects was irregular. The use of three of the first sort was to encourage freshness of judgment, each having its particular irregularity. Cards were but rarely remembered, practically never except in the case of groups differing widely in number. So far as the observers could tell, judgment was formed afresh in all these cases. In later experiments eight cards were used. This number was in the interest of avoiding the distribution-error. At first it was thought sufficient that all the groups should be merely irregular. Later it became evident that discrimination was very fine here and so that this factor must be eliminated by the usual precise method.

The space- and time-errors, where likely to be present, were eliminated in the usual way by performing an equal number of experiments with the groups in reversed arrangement. Several methods of doing this were at first tried; but these were all abandoned in favor of the following: The experiments were arranged in sets of 24, in each of which the proportion of kinds of cards was kept as indicated above. Each set with one space- or time-order of the groups was repeated with that arrangement reversed.

A word must be added as to the arrangement of results in the tables. Judgments of equality upon objectively unequal groups are entered as overestimations of the smaller groups. The per cent of correct judgments is equally divided between the two other classes, and for this reason that interest centres, not in correctness at all, but in the difference between the tendency of error in one direction and that in the other direction. No doubtful judgments were admitted,

but in such cases another trial was allowed later, usually when the observer was not aware that he was being given a new chance. The subjects are divided into three classes according as the results show a tendency to favor one or the other group or no tendency either way. A difference of 10 % is arbitrarily taken as significant.

1. *The Influence of Group-Area.* The Two-Group Apparatus was employed. The four sets of experiments carried out with this factor differed primarily in the material upon which the observer's judgment was based, and secondarily in certain matters of method. The attempt in them all was to approximate more completely to the isolation of the factor under investigation. They are numbered in the order of approximation. As marked results were obtained from each, they have all been offered for consideration in the four parts of Table I. A description of the material used in each case follows.

A. Squares (1 cm.) Neutral Gray no. 1. (Bradley), arranged irregularly in two groups with irregular outlines on a background of black cardboard. One group was large in area, the other small, the attempt being made to fill each space homogeneously. Groups were not proportional in shape of area.

B. As above, save that circles (11 mm. approx. in diameter) were substituted for squares, in the interest of distinctness for the several objects.

C. The area of the groups was oblong and regular, and the sides were proportional. (Compact 72.5 mm.: 58 mm.; scattered 110 mm: 88 mm. These relations were determined by the size of the frame that had already been used and by the desire to make the difference in area as marked as other necessary conditions would admit.) Each area was marked by a circle in each corner. The color of the compact group was the deepest shade of normal gray (Prang Normal Gray Darker); of scattered group the next higher shade (Normal Gray Dark). These dark grays were used in order to reduce to a minimum the tendency to produce after-images. The difference in the shades of the two groups was in the interest of avoiding the greater brightness due to the mass-effect of the compact group.

D. As in *C*, except that India ink outline circles ($\frac{1}{3}$ to $\frac{1}{2}$ mm. line) were used on a background of granite cardboard. This change was made to avoid, as far as possible, the greater mass-stimulation due to the reënforcing effect of the compact arrangement. The size of circles remained as before.

TABLE I

	A		B		C		D	
	274 *experiments with each subject*		248 *experiments with each subject*		120 *experiments with each subject*		132 *experiments with each subject*	
	Number of subjects	*Av. % of difference in favor of*	*Number of subjects*	*Av. % of difference in favor of*	*Number of subjects*	*Av. % of difference in favor of*	*Number of subjects*	*Av. % of difference in favor of*
Small	5	31.5	10	34.6	7	44.1	10	46
Large	3	35.1	4	44.1	5	41.5	3	26.3
No tendency	1	8.8	2	4.2	4	5.7	3	6.6

The per cent recorded in the no-tendency class is an average of *all* per cents below 10, whether in favor of the one or the other of the two remaining classes. This is true for all the following tables.

The following facts are presented by the several parts of Table I: (1) The large per cents of difference show that area is to a large extent a determinant of the judgment of relative number. (2) Different subjects show opposite tendencies. (3) A comparison of the results of individual subjects through the four series shows that this opposition in tendency occurs in the same subject at different times. The introspective notes of one of these subjects show the internal process of change from one tendency to the other. It consists in a gradual increase of analytic activity toward the compact group. At first glance the composition of the scattered group was more evident; but when attention was fairly turned toward the compact, the inability to isolate objects made them seem very numerous. The importance of a coöperating subjective factor is here evident. (4) Out of a possible 57 cases there are but 10 showing no tendency.

2. *The Influence of the Internal Arrangement.* As before, the Two-Group Apparatus was employed; and the factor was studied in three aspects.

A. The material consisted of two groups of gray circles (Normal Gray Darker, Prang) covering equal areas. In one group this area was filled homogeneously, in the other the circles were gathered into nuclei. In order that there might be exactly the same relation of parts when the cards were reversed, each group was so arranged on a diagonal axis of symmetry from upper left to lower right corner that each half repeated the other in reverse order. Otherwise the arrangement of circles was irregular.

Six cards only were used, — four (20 to 20), one (17 to 23), and one (23 to 17). Slight differences among them occurred in the arrangement of the equality-cards, which might help to counteract

any incipient reasoning from sameness of appearance to sameness of number. The large increase in the difference-values is accounted for in part by the fact that the cards (19 to 20) and (20 to 19) were omitted, and for this reason: that when an observer tended largely to favor a particular group, the introduction of a card in which that group was objectively greater would mean an increase in the number of correct judgments; whereas the introduction of two objectively equal groups for the others would increase considerably the number of erroneous judgments.

B. The numerical character of the cards here used shows a return to the usual. The material was like that of *A*, except for a new internal arrangement. Here the area of one group was filled homogeneously, while that of the other contained a pattern of this sort: An ellipse just contained within the boundaries of the normal area; a circle in each of the four corners of that area; and in the centre a diamond formed of four circles. Numerical changes were always confined to the ellipse, as less open to counting than the rest of the figure.

C. Material and method repeat *B* but with another internal arrangement. One group, as before, showed an area homogeneously filled, and irregularly, as usual. The other group carries to an extreme the distinction of open and filled space made prominent in the other groups of this table by massing the circles in an outline completely enclosing the area and in a diagonal from upper left to lower right corner. The outline did not show even spacing; more circles were crowded in one part than in others, that counting might be more difficult.

That there might be no attempt to remember cards, in all cases where there were twenty objects in the homogeneous group the same kind of irregular arrangement was repeated. This is a different method from that employed in *A*. Since the length of exposure was so short and the arrangement in the group irregular, either one is probably as good as the other.

D. The material used for *B* and *C* had a kind of regularity, since definite patterns were used. The introspection of the observers showed, however, that the patterns as such were not in question in the judgment, but rather the vacancies and the crowding. With the Two-Group Apparatus an arrangement in parallel lines could rather easily be counted, but with the One-Group Apparatus and its means for instantaneous exposure this difficulty was to some extent overcome. The arrangement of the objects in parallel lines

was therefore adopted and matched against the irregularity of an accompanying group. The same size of group-area was kept, but the small difference-cards were omitted. There was no other change beyond those made necessary by the apparatus and already indicated on an earlier page. The length of exposure was $\frac{1}{25}$ sec. The bearing of this time-factor on the results will be considered in a later section.

TABLE II

	A			B			C			D		
	132 *Experiments with each subject*			132 *experiments with each subject*			132 *experiments with each subject*			88 *experiments with each subject*		
	Homogeneous	*Nucleated*	*No tendency*	*Homogeneous*	*Pattern*	*No tendency*	*Homogeneous*	*Outlined*	*No tendency*	*Regular*	*Irregular*	*No tendency*
Number of subjects	10	2	2	10	1	3	7	5	2	2		1
Av. % of difference in favor of	53.23	1.5	6.1	35.7	52.8	5.3	42.7	34.8	7.9	·40.3		1.2

The several parts of Table II give us the following facts: (1) The judgment of relative number is very markedly a function of the internal arrangement. (2) The marked tendency among the observers to favor the homogeneous in *A* and *B* meets a check in *C*. Recalling the direction of difference between *C* and the other sets, that in *C* the gradually increasing contrast between the inner vacancies and the filling reaches a maximum, we may suspect that these vacancies begin to seem no longer a part of the group-situation, while the compactness of the filling, where it does occur, is thrust prominently forward. The notes of the observers confirm this suspicion. (3) The results of the different subjects show that the shifting of tendencies occurs as before. (4) As to the way in which regularity functions in the judgment, the notes of one observer are very clear. The blank spaces in the irregular are noticeable, he says, which is not true of the regular, where, on the contrary, one has a feeling of compactness of figure. I am able to confirm this character of the spaces by my experience outside this experiment. A simple pattern is very easily apprehended and irrelevancies of the background dismissed. Increase its complexity to a maximum, as in the case of an irregular group, and I am almost at a halt to isolate objects from their fellows and maintain them apart, yet together. The background is hardly to be shut out. This is probably due to the absence of a centrally excited image of the group. The object and the not-object run together. (5) The position of the single observer in the

no-tendency class of *D* was marked subjectively by great difficulty in forming a judgment. The groups seemed incomparable, the vividness of form excluding the perception of number.

3. *The Influence of Complexity in Group-Composition.*

Complexity of group-content was attained by introducing objects of different colors; so there was not a clean isolation of factors. By comparing these results with those recorded in Table IV, A, we shall be able somewhat roughly to make allowance for the factor of mere color.

Sets of 132 experiments each from sixteen observers were obtained for each of these factors. Unfortunately the distribution-error was not eliminated. Later experiments showed the importance of this factor, and, in consequence, the impossibility of interpreting the results under consideration. So new experiments were performed under the proper conditions, but at a time when only a few of the first observers could be used. Their results from the earlier series are given in Table III, A. The exclusion of the small-difference cards from the later series (Table III, B) and the consequent increase of the number of experiments on objective equality prevent comparison.

The material in *A* consisted of two groups of circles of the usual size, one Normal Gray (Prang), the other of three colors — Red, Yellow Orange Shade 2 (Bradley), Light Blue Blue Green (Prang). The intent was to equalize the two groups in brightness. When the observers were questioned about the relative brightness, supporters were found for all three possible opinions. So it seems probable that the groups did not differ widely in this respect. As nearly as the number-condition would allow, the three colors were represented equally in the group; and they were distributed so as to make the whole as homogeneous as possible.

In *B* the changes were the correction for distribution as described in the introduction to this section, and the substitution of equality-cards for those of slight numerical difference. In addition, three other colors replaced those of *A*, in the interest of regulated brightness and more pleasing æsthetic effect. These were, in the Bradley system of broken spectrum scales, A-Red, medium; A-Yellow Orange, dark; A-Blue Green, dark. With these exceptions *B* was like *A*. The observers were all inclined to consider the gray brighter than the mixed. The Two-Group Apparatus was used.

TABLE III

	A			B		
	132 *experiments with each subject*			132 *experiments with each subject*		
	Gray	*Mixed Colors*	*No tendency*	*Gray*	*Mixed Colors*	*No tendency*
Number of subjects	3		1	3		1
Av. % of difference in favor of	17.7		2.2	19.9		6.8

The following facts may be gathered from Table III: (1) The tendency to overestimate the gray is due in part at least to the additional factor of complexity in the other group, as is shown by the markedly changed tendencies in Table IV, A, where a solid color takes the place of the mixed colors. The actual colors involved in the two cases are different, to be sure, and necessarily so, and this difference may of course be invoked as the cause, as well as a possible difference in brightness between the gray and mixed in III, B. The introspective notes help us here. One observer felt that he favored the gray primarily because there was a tendency to consider but one color in the mixed. Another was drawn toward the gray because it seemed definite and consistent. For both of these observers æsthetic elements were involved in favor of the gray. The latter found also that the greater brightness of the gray gave it a larger area. With a third subject the fact of variety was felt as decidedly important; but his notes show a conflict between this factor and that of distribution which was the conscious basis for his normal judgment.

4. *The Influence of Differences in the Kind of Objects.*

A. The Factor of Color. The material in A 1 consisted of two groups of circles of the usual size, one Normal Gray (Prang), the other Red (Bradley). The attempt was made by this choice to equalize the brightness. The size and shape of the group-area were those of the smaller area of Table I, C and D. In A 2 the only changes were the correction for distribution, as described in the introduction to this section, and the substitution of equality-cards for those of slight numerical difference. The Two-Group Apparatus was used.

The following results appear in Table IV, A 1 and A 2: (1) While complexity seemed on the whole to diminish apparent number, red noticeably increases it. Some of the observers report that group as more vivid and interesting. One observer compensated by emphasizing the gray in attention, as his results showed. (2) If one ask how the color red functioned in the judgment, the reply must appar-

TABLE IV

	A 1			A 2			B			C			D 1			D 2			E		
	132 experiments with each subject			*132 experiments with each subject*			*132 experiments with each subject*			*132 experiments with each subject*			*88 experiments with two subjects 44 experiments with two subjects*			*132 experiments each*			*88 experiments with each subject*		
													exposure $= \frac{1}{25}$ *sec.*			*exposure* $= \frac{1}{4}$ *sec.*			*exposure* $= \frac{1}{25}$ *sec.*		
	Gray	*Red*	*No tendency*	*Gray*	*Red*	*No tendency*	*Large*	*Small*	*No tendency*	*Circles*	*Squares*	*No tendency*	*Simple*	*Complex*	*No tendency*	*Simple*	*Complex*	*No tendency*	*Bright*	*Dark*	*No tendency*
Number of subjects		3	1		3	1	8	3	5	8	4	4		3	1		2			3	
Av. % of difference in favor of		18.3	5.4		30.3	9	33.6	27.5	6	28.4	18.8	3.5		30.7	5.6		21.2			47.4	

ently be, by its brightness and vividness. The mixed group functioned in a double way, as vivid and so more numerous, as fragmentary and so fewer.

B. The Factor of Size. The Two-Group Apparatus was used, the material consisting of India ink circles (⅓ to ½ mm. line) on a background of granite paper. This paper was chosen here and for the experiments of Table I, D, to get a suitable mean between too sharp contrast and sufficient distinctness. The circles in the one group were ten mm. in diameter; in the other seven mm. The two areas were approximately equal, and of the same size as that of the more compact group in Table I. This is in fact the standard size throughout these studies in Relative Number, wherever area is not in question. The small-difference cards were included.

Two sources of possible complication must be considered. It is unavoidable that the factor of differences in compactness should enter and that clean results on the basis of object-size be denied. Our interpretation must not fail to consider this fact. Because of this, it seems unlikely that a distribution-error should arise; so the usual precaution to eliminate it was omitted both here and in the study of area (Table I). Distribution affects the appearance of the vacant spaces. When differences in the *amount* are by the conditions inevitably prominent, differences in the *conformation* may be safely regarded as of minimal vividness.

The following results appear in Table IV, B: (1) The illusion of numerical inequality is marked for many subjects. (2) The judgment is quite possibly a function of the two factors — object-size and group-vacancies. If we recall the fact that the small-object group is more scattered than the other, we shall note that the leading class here is like the leading class in Table I, and we may fairly reckon this factor as of importance in the issue. Of the incomplete introspective notes on this question, those of only one observer speak clearly for the size. He says: "There is an overpowering feeling of predominance in case of the large and I must judge for them. The large space covered seems an important factor. The longer I reflect upon the relative numbers the more numerous seem the larger, that is, they appear to increase over the small after the exposure. It is hard to give judgments of equal in most cases."

C. The Factor of Form. The material consisted of a group of circles, each of the same size as in former material; and a group of squares, each approximately equal to a circle of the other group. These were made of Prang's gray paper (Normal Gray Darker) and

pasted upon a black background. The areas of the two groups were approximately equal. The squares were set irregularly except for those in the corners, where the edges were placed parallel to the edges of the card. The Two-Group Apparatus was used and the small-difference cards included.

In this material, again, the formal elimination of the distribution-error was not attempted. The striking difference in the conformation of the vacancies through the form-differences of the objects probably makes the repetition of the exact positions insignificant. Still the fact must be noted.

The results appear in Table IV, C. (1) The illusion of numerical inequality is here again marked for many observers. (2) The introspective notes are not on the whole very illuminating as to the basis of judgment. One observer, who favored circles, found that the appearance of more orderly arrangement in squares made them seem few. Another, who favored squares, found, on the contrary, the more regular the more numerous, and thought that the squares may have seemed more regular. A third, who favored circles, found the squares better individualized, with whom a fourth agreed in both respects, who also was influenced by the apparently greater bulkiness of the squares. Fewer could go into a given area. A fifth, on the other hand, who found the circles better individualized, still favored them. So we have these observers apparently doing the same thing under opposite conditions, and the opposite thing under the same conditions. Here indeed is a situation for any theory. So far as we can learn from the foregoing, the form may influence the judgment merely through its space-characteristics, but possibly also through the vividness of intrinsic interest.

D. The Factor of Complexity. The One-Group Apparatus was used in this work and results were obtained for two different lengths of exposure, $\frac{1}{25}$ sec. and $\frac{1}{4}$ sec. The material differed, in that to the centres of the circles of one group were added small Red (Bradley) circles (6 mm.). With the apparatus used, the color was not very effective, the brightness contrast between dark centres and white periphery being chiefly prominent. The total group-brightness was of course diminished by those centres. The small-difference cards were omitted.

The results are recorded in Table IV, D 1 and D 2. (1) The illusion is apparently strong. (2) The amounts of the difference-values show that the shorter exposure is more favorable to the illusion. (3) The introspective notes indicate that both brightness and com-

plexity functioned in the judgment. Two observers, both of whom show large tendencies, were not conscious of any influence of complexity. One of these did find differences in brightness important; and in favoring the darker group his results exactly coincide with those of Table IV, E, where this factor is under direct consideration. A third found the complex group interesting. With a fourth the complex group developed in number amazingly during the few moments after exposure and had an appearance of great intricacy, often seeming to be in active movement. A fifth observer too felt that its numerical character depended on its complexity.

E. The Factor of Brightness. Hitherto the absolute arrangement of the objects in any two groups compared, where this factor has not been the object of enquiry, has been in the two cases different, though with respect to irregularity alike. This course was governed by a desire to avoid the substitution of a form-judgment for one on number, through recognition of the fact that both groups had identical forms. The resulting distribution-error I tried to eliminate in the usual way. Tests toward the end of these studies showed that there was no danger from this source. Errors seemed about as frequent as before. No observer made any comment on the fact, except one who through his official connection with the laboratory work knew that the test would be made sometime, but not exactly when. During many of the experiments he did not perceive the likeness of form; and when he did the numerical judgment arose without connection with that factor, as was shown by the feeling that the two groups were unequal in number. He called the relative fewness of the first group a case of "perspective effect." This must have significance for any account of the time-error; but by no means carries with it its own interpretation.

One welcome result of these tests was their assurance that I might without fear further simplify the experimental conditions by avoiding the possibility of a distribution-error. The material for these experiments on brightness therefore profited by this possibility. Each card had a different specific irregularity, but always in duplicate. In choosing the degree of brightness-difference Prang's brightest shade of normal gray was found as dark as could be conveniently perceived with the artificial light of the One-Group Apparatus. The contrast between this shade and white was quite evident enough for the purpose. The small-difference cards were omitted.

Table IV, E, shows the decisive character of the results. The observers fall all into one class in favoring the darker group, and by

a large difference-value. The following introspection of one observer shows the extent to which the factors of brightness and number fuse: "I frequently lose sight of time-order. It is a question of number and not one of light-intensity, and if called upon to state which group came first I might not be able to answer. In equality-judgments the difference of light comes out distinctly."

5. *The Influence of Complexity of Environment.*

The material prepared for these experiments certainly lays stress upon *relative*, not *absolute*, complexity; for the conditions were satisfied by placing 5 mm. strips of white paper, equal in length to the width of a group, a few millimetres off at the top and the bottom of the groups that were on one side of the cards. The One-Group Apparatus was used and the small-difference cards omitted.

TABLE V

44 experiments with two subjects.
88 experiments with two subjects.

Exposure $= \frac{1}{25}$ sec.

	Simple environment	*Complex environment*	*No tendency*
Number of subjects		2	2
Av. % of difference in favor of		15.9	8.5

The results are recorded in Table V. (1) The drift of tendency is toward the group with the more complex environment. No one markedly favors the other group. (2) The notes of the observers indicate that the added strips functioned through their effect upon the apparent area of their group. The observers all found the dimensions increased; but with some, apparently by contrast, the added height brought out sharply the narrowness. One observer found this true in general; another, when the barred group came first. The latter says: "The unbarred group, coming first, appears to reflect its compact character on the barred one, when it comes, so that it does not look so attenuated and strange." Here the image brought over to the second took the width of the second somewhat out of relation to its illusory height, whereas in the reverse order the contrast relation was fully maintained.

III. The Influence of Factors presented in Other Sense-Fields by the Objects whose Number is in Question

A very simple apparatus was employed. The objects whose number was in question were bright steel balls ($\frac{3}{8}$ inch) thrown loosely into square black frames, 13 cm. inside, placed side by side on a black-topped table. The experiments were performed in series of 30. The groups were kept equal numerically, with this exception, that into each series were introduced four experiments where the groups were so unequal that the observer could have no question as to the correctness of his judgment and the existence of objective differences. This numerical superiority was given to each group alternately, and judgments on it were, of course, excluded from the results. The actual numbers employed in a series varied between 35 and 60 in accordance with the following scheme:

1. 50 each	11. 55 each	21. 55. each
2. 45 "	12. 60 "	22. 60 "
3. 50 "	13. 40 to 60	23. 60 to 45
4. 55 "	14. 50 each	24. 50 each
5. 60 to 40	15. 45 "	25. 45 "
6. 50 each	16. 40 "	26. 50 "
7. 45 "	17. 35 "	27. 60 "
8. 40 "	18. 40 "	28. 55 "
9. 45 "	19. 45 "	29. 50 "
10. 50 "	20. 50 "	30. 45 to 60

The time of a single exposure — in this case two groups at once — was 3 sec. measured by a stop-watch. As to the arrangement of the balls, care was taken that they should not be massed in one place, but scattered somewhat homogeneously over the space within the frames. The illumination was daylight, so managed that shadows cast by the balls were reduced to a minimum. The observer sat close to the table with the groups directly in front of him. He either kept his eyes closed between experiments or held a small screen before them. Sometimes he merely turned away. The operator worked from the opposite side of the table, taking care to make the necessary noises as little suggestive as possible. The observers agreed that they were not consciously influenced by the manipulation.

The progress of these experiments disclosed an astonishing space-error. So far as was conveniently possible the usual technique of elimination was employed.

1. *The Influence of Active Pressure.*

In this study the groups were differentiated in this way: With one hand the observer rolled the balls of one group under his fingers, while the other group was presented to vision only. The method of observation consisted in rapidly and lightly rolling the balls under the fingers a few times and then surveying both groups visually for the remainder of the exposure, judgment being given on the visual number.

Evidently there is much that is rough about this procedure. Pressure and kinæsthetic factors are lumped off together; the length of the touch-stimulus was not exactly determined; and there is the possibility that the visual stimulation from the group touched is weakened. To be sure the method prevents any great difference in the latter respect; and if we are guarded in our interpretation, something of interest may be learned.

There appeared to be no convenient way to eliminate the space-error. The right hand was used with the right group and the left with the left. So here again interpretation must be circumspect.

TABLE VI

	A			B			C		
	52 experiments with each subject			*260 experiments with one and 208 with the other subject*			*145 experiments with one and 260 with the other subject*		
	Touch	*No touch*	*No tendency*	*Uneven*	*Even*	*No tendency*	*Weight*	*No weight*	*No tendency*
Subjects			2		1	1			2
Av. % of difference in favor of			7.6		10	1			2.4
SPACE-ERROR									
	Right	*Left*	*No tendency*	*Right*	*Left*	*No tendency*	*Right*	*Left*	*No tendency*
Subjects	1	1		1	1			1	1
Av. % of difference in favor of	69.2	23		30.8	20.2			32	4.4

Turning to the results in Table VI, A, we find the following: (1) The influence of the pressure-kinæsthetic complex practically does not appear; while the space-error shows a marked tendency that, for the two observers, is in opposite directions. (2) On the other

hand, the notes of one observer show that in his case at least the face value of the table is erroneous. To this effect he says in substance that he can make a more accurate estimate of the number in the group touched. He tries to ignore these sensations of touch, but with ill success in the case of the left hand, where clumsiness not only makes it difficult to touch the balls gently but also to keep them under the fingers, which often feel the ground-space. For this cause the group seems small in number. Clearly enough, then, it is the space-error that tells the story of the effect of the added stimuli on this observer, only it must not be interpreted as space-error. The pressure functioned in the judgment through its numerical aspect. But the positive effect with the right hand was turned to a negative with the left through its emphasis of vacancies. The high difference-value in the space-column becomes thus a striking evidence of the effect of pressure, and the results are accounted for without reference to the kinæsthetic factor. The other observer felt that the active pressure was relatively indifferent. (3) The entire absence of correct judgments on the objectively equal groups shows to what a surprising extent other factors have modified the numerical.

2. *The Influence of Unevenness in Active Pressure-Feeling.*

In the experiments of this section the groups differed in this way, that one rested on the smooth table-top while the other had for its bottom a coarse wire mesh covered with black cloth. The balls of the one rolled smoothly beneath the fingers while the other balls moved lumpily over their mesh. Both hands were used — each for the group on its side; and the method of observation and length of exposure agreed with those conditions in the preceding section, except that the balls were rolled a little more vigorously that the factor studied might come clearly into consciousness. The groups were interchanged for half the number of series. This could not of course completely eliminate the space-error, since kinæsthetic differences in the limbs remained uncompensated. In general the criticism in the preceding section is again applicable.

The results appear in Table VI, B. (1) One observer shows a tendency to favor the smoothly rolling group, while the other again shows no tendency. Both have large space-errors of the same character as in A of this table. (2) The introspection of the observer showing no tendency is to the effect that touch plays little or no conscious part in the situation. The other's notes give no hint that the factor studied here was influential; but to the effect on the judgment of touch in general, especially with the right hand, they give clear wit-

ness. The touch-sensations, he says, were difficult to ignore. Those from the right hand were more vivid than those from the left; and the right hand seemed more sensitive. Judgment was based on a general feeling of "moreishness" which came promptly. There is nothing to contradict the evidence of the earlier experiments that touch is again influential through its numerical character. (3) Both observers regard factors of distribution as of fundamental importance, though one was inclined at first to insist that there was nothing but number in his judgment. The significance of this unanalyzed feeling will appear in a later section. (4) These results agree with the preceding in the approximate exclusion of correct judgments.

3. *The Influence of Active Weight.*

The variation here in question consisted in lifting one of the groups during judgment of the relative number in the two groups. The apparatus was made by transforming into trays the frames containing the balls, by putting into these frames wire-mesh bottoms covered with black cloth. They were set each upon four small wooden pillars so that the hand could be easily thrust under the tray. At the signal a given tray was several times raised a little way and lowered, and the judgment formed on the same factor as before. Here again the space-error was not entirely eliminated. Each hand was used with the group on its side, but kinæsthetic differences peculiar to each of the limbs remained. There was always some motion among the balls in the lifted tray, though the gentleness of the lifting prevented the existence of much. This is a radical defect, but one not easily avoided with maintenance of other desirable conditions. Even more serious, as the issue proved, was the failure to control the lifting impulse; yet, as it happens, we are not prevented from getting an experimental answer to our question.

The results are recorded in Table VI, C. (1) They show no apparent effect of the weight, and with one observer the further unusual fact of no space-error. This error is marked enough in the case of the other, and, conforming in direction to that of the preceding sections of this table, allows us in so far to adopt the same interpretation of his results. (2) The introspection of one observer was to the effect that he felt a tendency toward a modification of the number-judgment by weight. It was especially strong when the group was lighter or heavier than was anticipated, the light group seeming less numerous, and the heavy group more. Occasionally he caught himself weighing the second group mentally; and sometimes he had to

recover himself from a tendency to make judgments on a wrong basis, presumably that of mere weight. With such a conflict of tendencies the character of the results is not surprising. Particularly important are the opposing tendencies lying in the factor of weight itself. The other observer reported that a very heavy weight exerted an influence that it was hard but not impossible to ignore, while a lighter weight did not effectively enter the situation at all. His earlier inclination to say that there was nothing but number in his judgment inclines one to believe that fusion of factors may have passed beyond the stage of ready analysis. (3) Our analysis has given us reason to believe that active weight has a definite tendency to modify the judgment of relative number.

4. *The Influence of Muscular Strain in Observation.*

The study was made from the point of view of more than one set of experimental conditions, viz.:

(1) Equal strain (minimum).
  (*a*) Right — left.
  (*b*) Up — down.
(2) Equal strain (maximum) eyes turned.
(3) Strain *vs.* ease.
  (*a*) Head and eyes turned.
  (*b*) Eyes turned.

The conditions of (1) (*a*) were exactly those of the earlier experiments with the exception that the groups were undistinguished save by position. In (1) (*b*) one group was so placed between the other and the observer that there might be as little increased effort as possible in viewing the farther. In the up-down movement more muscles are involved in the lift than in the fall of the eye. So really we have here a case of (3) but not so marked. In (2) the groups were put to the right and left at such distances that, when sitting between, the observer could just take each one in without turning his head. This brought a decided strain upon the eye-muscles. In (3) (*a*) the groups were separated by the length of the table — a distance of 90 cm.; and the observer placed in alternate series before each; as he was in (3) (*b*) where the farther group was carried to the limit of vision to be reached without turning the head. Here the strain was like that in (2), but for one group only.

An incompleteness in experimental analysis lies in the impossibility of separating the factors of distance and strain.

TABLE VII

	A		B		C		D[1]		E	
	Baldwin 46 experiments Hutchison 78 experiments Equal strain (minimum)		*52 experiments with each subject Equal strain (minimum)*		*52 experiments Equal strain (maximum) Eyes turned*		*104 experiments with each subject Head and eyes turned*		*52 experiments*	
	Right	*Left*	*Lower*	*Upper*	*Right*	*Left*	*Ease*	*Strain*	*Ease*	*Strain*
Subjects	*Baldwin*	*Hutchison*		2		*Baldwin*		2	*Baldwin*	
Av. % of difference in favor of	30.4	28.2		68.3		71.2		52.4	80.8	

Here are the facts of chief interest: (1) The following tabulation gives us a ready view of the character of the results in Table VII; and shows the extent to which they are consistent:

	A	B	C	D	E
Baldwin favors	right	upper, strain	left	farther, strain left	nearer, no-strain left
Hutchison favors	left	upper, strain		farther, strain left	

(2) The only inconsistency in the strain-distance complex is with Baldwin in E. He reported that the more distant group appeared rather as an undifferentiated mass whose number was not so well obtained, while in the near the individuals were significant. He seemed to be in the midst of these. The case seems analogous to that of the observer whose introspection was reported under Table I, and who at first accepted what we may call the objective analysis, by which the scattered group gave up more distinct objects than the compact; but later attempting voluntarily to disintegrate the compact, found a bewildering confusion in the task that made this group seem very numerous, and brought about in the end an exact reversal of tendency. (3) Can we now separate in the results between the influences of strain and of distance? So far we have regarded them as one complex. But the introspections speak merely of the space-characters of the objects, Hutchison agreeing with Baldwin that the more remote group is judged as an area rather than as a collection of definite objects. (4) The almost entire absence of correct judgments in these experiments adds new evidence to that of the immediately preceding experiments in proof of the insignificance of the actual numerical relation for the judgment of relative number.

IV. The Influence of Factors outside of the Objects and in Other Sense-Fields

The One-Group Apparatus was employed, and cards in general corresponding to those where area was not in question,—white-circle groups equal in size and irregular in inner distribution, which was not duplicated on the same card, though the resulting distribution-error was formally eliminated in the usual way. The usual care was taken to fill the group-area homogeneously. The small-difference cards were retained at first; but on later discovering

the possibility of duplication a few supplementary experiments were added.

1. *The Influence of Touch.*

The apparatus employed to give the touch-stimulus consisted in a long lever attached to the armature of a small electro-magnet. In the end of the lever was inserted at right angles a wooden peg, cork-tipped. In view of the other conditions of the experiment a convenient spot for the application of the stimulus was found to be the forehead where it curves backward above the right eye. The apparatus was supported by rods and clamps upon a long upright steel rod set in an iron base and placed behind the chairs of the observers. The same rod carried a head-rest, designed not as a support but merely to show the observer that he had returned to the original position after he had bent forward to record judgment. Where two observers were used at once two sets of this apparatus were employed, with the magnets in a single circuit governed by a floor-button. The touch-stimulus was made to coincide as closely as possible with the appearance of a given group.

In view of the practical remoteness of this factor from the object of judgment the experimentation here took two forms, — one in which the observer was passive toward the touch-stimulus; the other in which the effort was made closely to associate the touch with the visual group by imagining the group to be responsible for the touch. For the passive method the touch was given irregularly now on the first and now on the last, but as many times on one as on the other.

For the active method, it was given always on the last group. This constancy was held to favor the active association of touch and particular group. The constant time-error was guarded against by experiments in which no modifying factor was introduced. A and B of Table VIII present the results of the passive and active methods respectively. C and D repeat A with duplication of groups, — C with the usual ($\frac{1}{25}$ sec.), D with a longer, exposure. These last sets were taken that the factor of touch might be studied when the objective conditions of the strong distribution influence should have been removed. It might prove that a factor swamped in the former situation might emerge into effectiveness.

The following summary gathers the chief facts of Table VIII: (1) Touch appears practically without effect in A. (2) In B, the results for touch seem again insignificant; but comparison with the control-results, to isolate touch from time-order, while it shows no marked

change for Angier, does show for the others that touch was effective in determining the direction of error by difference-values, in the two cases of 10.2 and 14 per cent. The active method seems to be slightly more favorable to the influence of touch. (3) The duplication of the groups in C gives a large increase to the apparent effectiveness of touch, which is considerably diminished but not destroyed by the lengthening of the exposure in D. (4) The introspection for A indicates that touch under these experimental conditions has little subjective importance for the judgment of number. It is sometimes quite unnoticed. Angier made a possible exception in its favor in cases of great hesitancy where it added "importance" to the group with which it occurred. Usually he felt little doubt. With Shaw the touch was at first distracting but later indifferent. Johnston's notes indicate rather more effect. The touch prevented strict attention to the figure impression whereby the space-intervals in that group lost in value. Later it lost its confusing effect. Here seems to be subjective tendency, but not enough to predominate in results.

TABLE VIII

	A			B			C			D		
	88 experiments with each of two subjects. 132 experiments with one subject			*198 experiments with each of two subjects. 110 experiments with one subject*			*44 experiments with each subject*			*88 experiments with one subject and 44 with the other Exposure = ¼ sec.*		
	Touch	*No touch*	*No tendency*	*Touch*	*No touch*	*No tendency*	*Touch*	*No touch*	*No tendency*	*Touch*	*No touch*	*No tendency*
Subjects			3			3	2			2		
Av. % of difference in favor of			3.2			5.7	26.1			13.7		

Results in B for the subjects separately were as follows: Angier 4 %, Johnston 8.2 %, Shaw 5 %, all, so far as they went, in favor of the touch-group. Control experiments to determine the time-error gave the following results: Angier 6.8 % in favor of the group last seen, Johnston 2.2 %, and Shaw 9 % in favor of the first group.

Some further introspective evidence appears in connection with the active method of B. Angier confirms his earlier account exactly. Usually the factors of distribution practically associated with number determine the judgment promptly; but in cases of doubt the touch is felt to add to its group something that appears as number-value. Johnston's subjective situation seems a little complicated. I may summarize thus: (*a*) The connection between the touch and its

group being established, that group seems smaller, as being, together with the touch, somewhere nearly equal to the first. (*b*) The connection established and touch failing to come, that group seems smaller. (*c*) The connection not established and attention being concentrated on the visual impression, the touch-group feels much larger. The curious attitude in (*a*) results in a discounting in advance of the actual number. This done, the touch adds numerical value to its group. In (*c*) the effort at abstraction appears to emphasize the second (touch) group. Later, he reported similarly that the touch-stimulus seemed to add to the number of circles in its group even when the judgment favored the other group; and that "any outside stimulus connected with the one of two exposures tends to lose its own significance and be translated into number of dots to help the accompanying exposure to equal or exceed the first." The touch-group is felt to have more significance through association with an idea of superior energy or greater motor impulse.

Of the character of the influence exerted by the touch, Shaw reported that there seemed to be a diminution in the size of the first group and something extra in the second. More specifically, this effect appeared at times as an added circle at the right of the second (touch) group. He thought that this effect was overruled by the real bases of number-judgment which he summarized as "size, regularity, density, etc."

These notes show a definite tendency on the part of the touch-stimulus to break in upon the course of the number-judgment ordinarily determined by the practical association of a specific group of factors with number. That this result gets no more marked registration in the percentages is apparently due to the strength of these customary associations.

(5) The extent to which the distribution-error complicates the present study is shown by the prompt increase in effectiveness of the touch-stimulus when the groups were duplicated, as in C and D.

2. *The Influence of Hearing.*

The scheme of the experimentation upon this factor conformed in general to that of Section IV, 1. But a new sort of differentiation was possible in the auditory field, and one more readily suggestive of numerousness, perhaps, in that by use of an electric bell a rapid succession of sounds could be given with one group while with the other a single sound could be produced. An actual numerical difference in the auditory field might fuse with the factor of relative visual number and determine the judgment to its direction. These results

are set down in B of Table IX. The same set of cards was used in the One-Group Apparatus for these experiments as for those of Section IV, 1. In these two sections of Table IX the observers did not know on which group the sound or the particular sound would be given; but any possible disturbing effect of this irregularity was formally eliminated as in Section IV, 1. The experiments of Table IX, C, repeat those of A with duplication of groups; and D repeats those of C with longer exposure.

The sound for A was that of a small organ-pipe (Ut 4) blown by mouth. As in A of the preceding table the observers did not know in a given experiment with which group the sound would be given, but, as before, it was given the same number of times with the first as with the last. For B the multiplied sound was produced by an electric bell with a wooden gong. This was adopted in preference to metal because of the prompt ceasing of the sound after the stroke, — a very necessary condition when this sound accompanied the first group, that it might be clearly connected with its own group. A metal gong was used for the single sound, that the two might not be too unequal in loudness. Its vibrations were deadened by a rubber band, and each bell was controlled by a floor-button. For C and D a higher sound, from the same pipe unstopped, was used in preference to the former, for the reason that in certain experiments performed just previously the lower sound had been used and was presumably very familiar. So in order that the sound might be brought, if possible, afresh to the attention, the change was made.

TABLE IX

	A			B			C			D		
	132 experiments each with 3 subjects. 180 with 1 subject			*44 experiments each with 2 subjects. 88 with 1 subject*			*44 experiments each*			*88 experiments each*		
	Exposure = $\frac{1}{25}$ *sec.*			*Exposure* = $\frac{1}{25}$ *sec.*			*Exposure* = $\frac{1}{25}$ *sec.*			*Exposure* = $\frac{1}{4}$ *sec.*		
	Sound	*No Sound*	*No tendency*	*Many Sounds*	*One Sound*	*No tendency*	*Sound*	*No Sound*	*No tendency*	*Sound*	*No Sound*	*No tendency*
Subjects			4	1		2	1		1			2
Av. % of difference in favor of			5.4	18.2		2.2	20.4		4.6			2.2

The results of these experiments may be summarized as follows: (1) The figures give evidence of but two cases out of eleven where sound was influential. (2) Duplication of groups is not effective in

developing evidence of the influence of sound. (3) Increased length of exposure works, as in former cases, to lessen the influence of the modifying factor. (4) The introspections are to the effect that the sound seems to be entirely without influence upon the judgment, beyond the distraction it brings in the earlier stages of work. Sometimes it dropped wholly out of consciousness. Sometimes the distraction seemed to last longer. One observer reported, when D was taken, that he felt as if the sound sometimes increased and sometimes decreased the apparent numerousness. In some other experiments not directly upon this point, but later to be reported, a sound was used; and one observer reported that it seemed to become functionally connected with certain gaps in the groups, as though the puff had blown a hole in the group. Here its effect was of course to emphasize negative factors. It appears thus that the sound might function in opposite directions at different times, somewhat in accord with the particular character of the visual presentation. We should expect, then, to have percentages that look insignificant. (5) We shall not have failed to notice the difference between touch and auditory stimuli in the feeling of influence upon the number-judgment. If we seek a cause for the superior influence of touch, we may perhaps find it in the fact that practical experience has trained us to disregard in any case of judgment such simultaneous presentations as were employed for auditory stimuli; while a definite tap upon the brow is a rather unusual experience likely to attract notice to itself in spite of attempts at abstraction. As one observer said, who took part in both kinds of experiments, the touch seemed more "intimate."

3. *The Influence of Kinæsthetic Impression.*

The method consisted in the employment of active effort upon a fist dynamometer or a wooden handle during the appearance of one of the groups. The handle was preferable because noiseless. The effort was made with the left hand because the right was used in recording. The amount of it was left to the observer's regulation, with the one instruction that its presence be made decidedly evident but without too great fatigue. The cards of Section IV 1 and 2 were used in the One-Group Apparatus. Similarly again the experiments were repeated with the duplicate-group cards. I present no table here because the figures show practically no influence of the effort. On one subject 176 experiments were made; on a second 132; on a third 88.

It is interesting to note here certain results obtained from one observer when he was in what he described as an active attitude toward

the groups, in which he seemed to rouse himself to an unusual pitch of concentration upon the visual situation. This was evidently a condition of increased effort to abstract. Without abstraction he gave 26 to 6 in favor of the effort while with abstraction this tendency had fallen off to 30 to 17. The strength of the tendency is thus strongly indicated. Another observer felt a kind of motor difference between the groups; he expected the effort-group to look larger and felt additionally excited, a scattered activity, while he was passive toward the other group. Perhaps this account puts a little meaning into his small per cent. That his power of abstraction was effective here is hinted by his remark that he felt a difference in the groups even when he judged them equal. The third observer found no subjective evidence that effort modified his judgment.

V. The "Errors" of Experimentation

Throughout the foregoing experiments has been involved the possibility of some one of the three "errors" of experimentation, those of time, space, and distribution, and sometimes all three. Their effect on the results, if it existed, was, to be sure, eliminated in the well-known way, but their existence, if actual, would raise an interesting problem. It was possible, in the case of every group of experiments, to rearrange the tables in such a way as to bring out the evidence for any tendency to overestimate, for instance, the first group as against the second, the right as against the left, or one kind of irregular distribution as against another.

The distribution-error must have a word of explanation. It refers to a tendency to give more wrong judgments in favor of one kind of irregular distribution than of the other kind with which, in a given card, it is mated. In the construction of a set of cards several forms of irregular internal arrangement were used, in order that the judgment might not be one merely of form, and of course on any given card the forms were not the same. Elimination of the effect of these form-differences from the results involved the appearance of any given one as many times in connection with one of the two contrasting factors studied in a given experiment as with the other. Thus two sets of forms were carried through an experimental series — a source of error indeed, but avoidable only by such means as were used to escape the effects of the space-error. Analysis would show which, if either, of the two sets received more judgments in its favor, resulting in further evidence as to the extent to which the judgment of relative number is a function of distribution, and as to the fineness of discrimination for such differences.

Now the tables, when thus rearranged, show that these errors exist to a surprisingly large extent. In many cases their causes, whatever they are, seem to be the controlling factors in the judgment of relative number.

Barring the experiments of Section III, in which the space-error has largely been accounted for, I now propose to gather in one survey all the results of those analyses that have given us the information of the existence of these errors, and all the material of later tables that bears on this point, and to test them by further experimentation. I will begin with the space-error.

TABLE X

	Cases	Av. % of difference in favor of Right	Cases	Av. % of difference in favor of Left	Cases	Av. % of difference in favor of No tendency
Angier	1	25	6	17.9	9	5.7
Davison	5	26.4	1	10.8	6	6.2
Dunlap			7	18.3	4	4.4
Holt	2	13.6	7	17.6	4	3.2
Hylan	8	23.6			7	6.7
Meakin	2	19.2	1	29.6	8	5.5
Meriam	2	16.5	2	12.9	7	6.8
Moore	2	11	3	16.6	7	3.1
Peterson	1	13.6	1	12.2	9	4.3
Rogers	4	21.9	2	15.9	6	4.5
Rouse	3	15.5			8	5.1
Shaw	3	20.9	5	18	7	6.1
Windate	1	22.8	4	19.5	7	4.5
Yerkes			6	26.6	8	5.2
Henry	1	10	2	16.6	3	8.1
Woods			3	19.3	3	4.8

1. *The Space-Error.*

Table X presents to us a summary of the values of the space-error tendency. (1) Taken as a whole they fall into all the three classes that are possible; (*a*) favoring right; (*b*) favoring left; (*c*) no marked tendency. (2) There is no observer that does not at some time show a fairly marked tendency. (3) All the observers fall into (*c*) and all but five into both (*a*) and (*b*). (4) More favor the left than the right, — 50 to 35. (5) This survey makes it clear that the observers agree neither with themselves nor with each other in the

direction of influence exerted by the causes underlying the space-error.

a. Special Experiments to establish the Facts. It might be suspected that irregularities would be more apparent where other factors such as we have been studying enter to complicate the situation from the point of view of pure relative position of the two groups. Table XI presents the answer to this query. The cards used contained groups of gray circles (Gray Darker, Prang) arranged in equal areas of the usual size and shape. The distribution-error was eliminated, though not by duplication, and the small-difference cards were retained. The Two-Group Apparatus was used, with an exposure of $\frac{6}{5}$ sec.

TABLE XI

88 experiments with each

	Right	*Left*	*No tendency*
Subjects	4	7	3
Av. % of difference in favor of	25.6	23.1	3.4

The results give us again our inevitable three classes, and in many cases a difference-value surprisingly large when we reflect on the simplicity of the conditions. That the omission of complicating features was of importance is shown by the fact that more of the observers (11 out of 14) show a marked error than in any other case. Clearly enough the various factors introduced tend to eliminate the space-error, but when in any case it does enter, it is even then capable of rising to as high a degree on the whole as in the uncomplicated series, as is shown by the fact that in but four cases does the new value surpass the best of the old, and in three of these by a trifling amount.

It is interesting to note that the three cases of minimum space-error show a well-defined tendency to be determined by distribution.

b. Possible Bases of this Error. The outcome of these special experiments is that the factors found in the groups are at least not directly responsible for the situation that we are considering. The divergence among the observers shows this. In hunting after the cause for this apparent influence of side, we look first for changes in the peripheral, and then in the central, processes that precede the judgment. The material used for the experiments of Table XI seems approximately

to have equalized all the objective factors in the two groups. How could there be anything further in the peripheral process whereby group could be differentiated from group? The most evident thing is that the visual stimulus is received in a different way from the two groups. There is a definite peripheral mechanism whose factors seem essentially to be two, however variously they may be combined: (*a*) The relative amount of time given to each group; (*b*) the order in which the groups are viewed.

The observers were instructed and continually reminded to equalize the amount of attention devoted to the group; but as this is not wholly a voluntary matter, the possibility of failure to conform has to be reckoned with. Experiment must therefore be employed to test the influence of these factors before one can fall back upon a central process as the cause for this tendency to favor a side.

c. Its Relation to Differences between the Groups in Length of Look. The material was the same used for the experiments of Table XI. The method was the same with the following necessary exceptions: The longer exposure was double the shorter ($\frac{1}{5}$ sec. to $\frac{2}{5}$ sec.), and $\frac{2}{5}$ sec. elapsed between the two. Further, the experiments were so arranged as to equalize the influence of the order of exposure with respect to both side and relative length. The means for effecting successive exposure took the earlier form described in the introduction to Section II.

TABLE XII

88 experiments with each

	Longer	*Shorter*	*No tendency*
Subjects		6	7
Av. % of difference in favor of		18	6.5

These facts are yielded by Table XII: (1) There are but two classes of observers, as no tendency exists to favor the group of longer exposure. (2) The time-error shows a considerably more marked tendency than the length of look, which is indeed somewhat surpassed by the space-error. (3) The persistence of the space-error, even among those that reveal a tendency in length of exposure, shows that the factor of relative difference in length of look cannot account for it. The persistence of it, too, when the order of exposure is controlled, even though the conditions are not wholly adapted to the study of this latter factor, suggest at least that the space-error is

independent of even that order; but into this we shall make special enquiry. (4) The judgment of number is independent of the amount of eye-movement devoted to the fixation of the objects in a group. This conclusion, so far as the actual movement is concerned, is established by the fact that so many observers favor the shorter look; and by all the experiments with the One-Group Apparatus where an exposure of $\frac{1}{25}$ sec. was used, since that time was too short to admit of movement. That ideated movement is likewise insignificant appears from the fact of marked error arising in the material where the groups were duplicates. Here no motives to different movements could lie in the material.

d. Its Relation to the Order in which the Groups are viewed. Table XIII gives us the results of the enquiry. The experimental conditions were not changed except as to the length of exposure. Each group was given $\frac{3}{8}$ sec.; and half the experiments were performed in the order right-left and half in the reverse order.

TABLE XIII

88 experiments with each

	First	*Last*	*No tendency*
Subjects	3	9	2
Av. % of difference in favor of	17.5	28.3	1.7

The results may be thus summarized: (1) The order of exposure is notably influential upon the judgment of relative number, giving the usual three classes, with the tendency to overestimate the last group well in the lead. (2) The persistence of the space-error under these relatively simple conditions shows conclusively that it is not a function of the order of exposure. The two are independent variables.

2. *The Time-Error.*

In pursuit of our enquiry we must survey the facts as they are given in the various experiments already reported and later to be reported. These facts are gathered into Table XIV, which furnishes the following items of significance: (1) All the observers, with the exception of Rouse, show at some time a definite tendency. One case only is given for him in this table, but other experiments not included in the tables from which the present is drawn confirm this fact by the

ratio 29 to 30. (2) There is a rather striking consistency in the several observers. (3) The predominance of the last group is marked.

TABLE XIV

	Cases	*Av. % of difference in favor of First*	*Cases*	*Av. % of difference in favor of Last*	*Cases*	*Av. % of difference in favor of No tendency*
Angier			7	19.1	4	4.9
Baldwin	4	20.5			1	3.4
Bell			1	18.2	4	6.3
Davison			2	31.2		
Dunlap	1	11.4			1	2.2
Holt			10	19	2	6
Hylan	2	16	5	25.4	5	4.9
Johnston	2	25.2	4	35.5	4	4.2
Meakin			2	39.7		
Meriam	1	29.6			1	2.2
Miller			3	17.1	4	5.5
Moore			1	11.4	1	2.2
Olmsted	1	15.2				
Peterson			1	17	1	9
Rogers			1	10.2	1	5.6
Rouse					1	1.2
Shaw	6	16.7			5	7
Windate			1	11.4	1	1.2
Yerkes			2	42.7		

a. Relation of the Error to the Absolute Length of the Total Exposure. Table XV is set to answer this question. It is unsatisfactory in that but two observers took part in both XIII and XV. The material used for judgment consisted of the same cards used in the earlier experiment, but presented now in the One-Group Apparatus. The time of exposure was changed from $\frac{8}{5}$ sec. to $\frac{1}{25}$ sec. for each group. As the space-error was eliminated, the tendency to a time-error, if present at all, would presumably have freer play.

But the difference-values of the new table are for the most part very small. We have thus the further fact about the time-error that, under the conditions studied, it appears to be independent of the absolute length of exposure, when the groups are equal in this respect. To this we may add another fact drawn from Table XIV, that with the One-Group Apparatus the time-error is greater on the whole where the groups are differentiated by other factors. Thirdly,

the values for Table XIII show that with all complicating factors withdrawn, except the differences in position, the error is at a maximum. This may be significant of the effect of space-differences upon that error, or, more probably, be due to the general difference between work by daylight and work in a dark room by artificial light. We shall be better able to consider this later.

TABLE XV

88 experiments with each of two subjects.
176 experiments with one subject.
154 experiments with one subject.
66 experiments with one subject.

Exposure = $\frac{1}{25}$ sec.

	First	*Last*	*No tendency*
Subjects	1		4
Av. % of difference in favor of	15.2		6.8

3. *The Distribution-Error.*

The last of our three "errors" of experimentation is now before us. We may recall once more the meaning the term has had for us in these studies. It points to a tendency discovered by the use of those cards where all objective factors were in the course of a series equalized, — a tendency to mass one's judgments in favor of a particular arrangement of the circles; though each group had been constructed with a view to filling the given area as homogeneously as an irregular arrangement would allow.

As in the two "errors" preceding, so here we must get possession of the facts that gave rise to the present enquiry. Table XVI presents them to us, gathered out of all the tables wherein such a tendency has been technically reckoned with. But first a few words of explanation are needed to make the new table intelligible. Two sets of results are found in its two parts. In each set the particular group-arrangements employed and the frequency of their appearance are exactly the same. The two sets differ, as their headings suggest, in that the material for the second set was formed out of the first by replacing the small-difference cards by those having equal groups. Such a change as this might affect the proportion of judgments given in favor of the two sets of arrangements in a particular series, and

these new results are, therefore, no longer fully comparable with the earlier ones. In presenting the directions of tendency in the results, it is impossible here, as in all the similar cases throughout the tables, to name a factor as a standard in whose favor all the judgments in the plus column should be understood as given, — impossible for this reason that, because the very method by which the circles were distributed in the groups, the experimenter was unable to satisfy himself as to the significant differences in the arrangements. All the results, however, when analyzed on this basis, were recorded consistently, so that consistencies and agreements among the observers might be readily apparent. We can now understand in part what Table XVI has to say to us.

TABLE XVI

	A						B					
	Cases	*Class 1*	*Cases*	*Class 2*	*Cases*	*No tendency*	*Cases*	*Class 1*	*Cases*	*Class 2*	*Cases*	*No tendency*
Angier			4	19.1	2	6.3					3	2.9
Davison			3	23.1								
Dunlap			2	13.7	1	2.2						
Holt	3	15.5	1	25	2	4.5	1	22	1	21.6	2	3.1
Hylan	1	11.4	2	14	3	6.7	2	50.8	1	11.4	1	8.4
Johnston			5	30.4								
Meakin			3	34.9								
Meriam			1	16	2	6.8						
Miller			5	32.1								
Moore	1	39.8			2	5.7						
Olmsted			1	27.2								
Peterson			3	42.1								
Rogers			1	11.4	2	5.7						
Rouse			2	14.2								
Shaw			6	29.1					2	26	1	0
Windate			3	30								
Yerkes			3	17.8								

Here as elsewhere the per cents recorded indicate the average per cent of difference in favor of a given class.

Here are the facts, first of A: (1) The only lapses from consistency are confined to two observers; and in both these cases there is but a single break in a uniform trend. (2) With three exceptions all agree in the trend of their difference-values. Of these three — Holt, Hylan, and Moore — the last furnishes but one significant value, and so must be left out of the reckoning on this point. (3) Of the 64 cases, 50 rise above 10%, some far beyond, showing the importance for the judgment of relative number of this factor of distribution. (4) Of the 50 cases, 45 agree in tendency. (5) That

with this surprising agreement we have still a few exceptions, adds another item to the growing array of evidence on behalf of the importance of some subjective factor for the number-judgment. As to the nature of this factor we are yet in the dark. (6) To these facts B of this same table adds the further information that the observers inconsistent in the old are not consistent in the new, while the consistent still maintain their record.

a. Analysis of the Experimental Conditions of Distribution. At once we are interested to enquire for the factors underlying these results. To put ourselves upon the right track we must first consider what factors are involved in any such arrangement of objects as we have used in the material for these studies, and then, more precisely, we may ask in what way such arrangements could differ significantly. Finally, by an experimental trial-and-error process, we may solve our problem.

The groups of objects in our material were arranged in an area marked out in each corner by a circle. Within this area the circles were set irregularly, with the result that the group, as a mass of objects distinguished from a homogeneous background, had a more or less irregular outline whose irregularity varied with different internal arrangements. Within its outlines this area presented a mixed pattern of bright and dark. While the total enclosure marked off by the corner circles was always the same and theoretically the relative amounts of brightness and darkness in equal groups was likewise the same, yet practically differences, more or less slight, might enter through the changing character of the rude outlines whose ideal completeness could scarcely be brought out of a black background by the uninitiated. The amount of this difference is sometimes surprising to one whose chief thought of the group has been as vignetted in process of construction. As the objects are pushed toward the edges the central spaces open out; as they are withdrawn toward the interior gaps appear in the margin.

It is not a very easy task to fill an area with objects in irregular arrangement in such a way that no sections of vacancy or filling stand out by contrast against the remainder of the same element. To succeed in this is to fill the area homogeneously. But the chances are good that some vacant patch will get slightly the better of its neighbors or some section of circles will gather a little more closely than the surrounding circles; or perhaps a gap in the outline will be unexpectedly intrusive. Now in a given area the circles of one part cannot become more thickly massed without a corresponding enlarge-

ment of the vacancies of the other parts, and of course the converse is as true; but this theoretical situation may be quite out of ken at the moment when the group is seen. Either member of this pair of complements may stand out vividly in the field and its fellow quite escape perception. The very nicety with which in practical affairs we have to make a reliable comparison of this sort shows what suspicion of accuracy the off-hand judgment has bred. And further, the widening of a gap or thickening of the filling in one small part of a group may give a complementary loss to the rest of the group small enough to be unperceived when distributed throughout the larger section.

Two factors must therefore be considered as possibly significant in moving the judgment, — vacancies and filling; and with the former must be reckoned indrawing of the outline. Psychologically, increase in the prominence of either of these factors would be all one with their objective increase. With respect to the direction of their influence upon the judgment of number the increase of vacancies must signify the waning, and the increase of filling the waxing, of the objective number in the group.

It is in advance altogether probable that the results gathered into Table XVI were brought about by these two factors, at least in large part. And we have also in these factors the possibility of two types; for as we saw above, increased vacancies in one part involves increase of filling in another, and conversely. So the interesting question turns upon the altogether disproportional representation of types. Which is the type of the majority?

b. Experimental Test of Hypotheses. The question was put to the test of experiment. This was done by using groups in which now vacancies and now filling were objectively emphasized in contrast with the usual homogeneous group. First the vacancies. A set of cards was prepared after the method previously used to eliminate the distribution-error without duplication of groups on any one card. (See Section II.) In the present case, however, the two sets of arrangements were definitely differentiated as already indicated. One set had a homogeneously filled area, the other a prominent vacancy within or gap in the edge. The size of these variations was kept pretty close to the limit of noticeableness, that the increase in compactness of the other portions might be as slight as possible. It was experimentally necessary to free the material as far as might be from ambiguity, and practically important to avoid rousing the suspicions of the observers and the resulting reflections. It seemed

very likely that the strength of the tendency shown by the distribution-error was due to its appearance in situations where the observers knew that other factors were being tested.

The general method already described was used in preparing the groups that gave objective prominence to compacted parts of the filling. To fulfil the conditions outlined above was here even more difficult than in the first set; and the cause will appear in the sequel. The small-difference cards were omitted and the One-Group Apparatus used.

A further attempt was made to head off reflection by a subterfuge. It had been found that, among the factors whose influence on the judgment had been studied, hearing had been as little effective as any. So the small stopped pipe used for those experiments was again brought into service and the error resulting eliminated in the usual way. Incidentally our new tables will thus give us further information about the effect of this factor, though of course under conditions that are theoretically highly unfavorable, since we are forcing upon the attention of the observers other factors that experience has shown them only too ready to seize upon. So if a tendency traceable to the factor of hearing should appear, we ought perhaps to give it somewhat more than its face value.

TABLE XVII

	A.			B.			C. 88 *experiments each*		
	Exposure = $\frac{1}{25}$ *sec.*			*Exposure* = $\frac{1}{4}$ *sec.*			*Exposure* = $\frac{1}{25}$ *sec.*		
	Homogeneous	*Vacant*	*No tendency*	*Homogeneous*	*Vacant*	*No tendency*	*Homogeneous*	*Compact*	*No tendency*
Angier	[1]50			[2]51.2			39.6		
Baldwin	[2]53.4			[2]55.6			35.2		
Bell	[1]52.2								3.4
Holt	[2]44.4				[1]13.6			27.2	
Hylan	[2]51.2			[1]52.2			39.6		
Johnston	[1]56.8			[2]62.6			44.4		
Miller			[2]4.2	[1]25				16	
Shaw	[1]29.6			[2]14.8					2.2

[1] 44 experiments.
[2] 88 experiments.

The per cents recorded indicate the average per cent of difference in favor of a given factor.

Now we are ready to inspect the results. Table XVII, A is the outcome of the attempt to emphasize vacancies. Its experiments with $\frac{1}{25}$ sec. exposure were repeated with one of $\frac{1}{4}$ sec. as Table XVII,

B, shows. In Table XVII, C, the emphasis of compactness is concerned.

For convenience we may again resort to a summary outline in extracting the meaning from these tables. First Table XVII, A. (1) All the observers but one agree in favoring the homogeneous, most of them with very high difference-values. (2) Miller alone gives no tendency, and his notes show a conflict between the increased vacancy and the increased compactness. In other words, his discrimination was too keen for the material. Under the circumstances he constitutes no exception to the conclusion that the vacancy objectively emphasized was the cause for an underestimation of its group.

From Table XVII, B, we learn the following: (1) All the observers save one favor the homogeneous group, in most cases by large values. (2) The difference in the length of exposure seems to have no significance for this tendency, since, while Holt and Shaw decline, Miller rises in the scale.

Table XVII, C, gives us these facts: (1) The difference-values have noticeably fallen off. (2) We have again the customary three classes, but with homogeneous leading as in the earlier tables. (3) By his present favoring of the compact, Miller has now appeared in all three classes, while Holt has developed the preference for the compact that was budding in XVII, B. (4) The presence of four well-marked preferences for the homogeneous shows that the vacancies in the compact group were more significant for the number-judgment than was the increased compactness of the filling, and that in spite of the experimental effort to the contrary. (5) The decrease of this tendency and the growth of the opposing, indicates that the judgment is determined in either case by the more vivid factor.

The conclusions to be drawn from these facts lie close at hand. (*a*) The results in Table XVI, with their disproportionate division into classes, were evidently due to the tendency of three observers to note the filling and of the rest to be concerned with the vacancies. (*b*) The judgment of relative number under these conditions is primarily a judgment of vacancies. (*c*) The subjective factor of vividness determines the direction of error, and may attach to either vacancies or filling, though it usually attaches to the former.

It may not be out of place here to speculate a bit as to the probable cause for so close a dependence of the number-judgment upon what has no number, so to say; upon an object that has no standing in the official conclusion. The situation seems to be fundamentally based upon the conditions that determine contrast. In a homogene-

ous field no part stands out. Introduce a small object quite different in brightness or complementary in color and the attention is drawn instantly to it, but internal differences in its content are quite lost in the common quality by which it differs from the ground. A case somewhat analogous is furnished by our material, particularly in the One- and Two-Group Apparatus. The small group is so unified by its contrast with the field that internal differences must be made out with relative effort. Now internal differences are necessary to the numerical character demanded of it, and they can be brought out in no way save by attending to the vacancies and so isolating parts in the threatening unity, each in a kind of space-matrix. The most careful observer could not do better on his way to truth; and that is why the error was so much larger when the factor of space-differences was studied.

That group is normally the more numerous in which the vacancies are less completely developed under observation. We say "normally" here by virtue of the speculation just completed as to the best method of attaining a judgment objectively true. For a man thus proceeding, our proposition is a sound statement of fact, to which the following results of our experiments bear witness. (*a*) The experiments recorded in Table XII on Relative Difference in Length of Look shows no exception of a value equal to 10 % to the general statement that all tendencies, when any existed, were in the direction of favoring the shorter group. The shorter the time of exposure the less completely would the vacancies develop. (*b*) Table IV, E, shows that without exception the darker group tends to be judged the more numerous. (*c*) Table XXI shows for each subject that in a shorter exposure the absolute number seems considerably greater than in a longer exposure.

No comment seems necessary to concentrate the force of such evidence. If we carry our proposition to the detailed results of our separate studies in factors of distribution, we shall find that it helps us to understand those few exceptions to the general trend of observers as they appear in Tables II and XVII. The exceptions there favored the groups in which compactness of parts went along with certain large vacancies. Possibly enough they refused to fall in with the objective analysis, and, disregarding the prominent vacancies, devoted themselves to a development of the vacancies within the compacted parts.

c. The Factor of Hearing. The time-error analyses of the experiments of Table XVII have already contributed their facts to the

special section dealing with that error. But one or two interesting facts have remained unnoticed in the sound-analysis. In the experiments of Table XVII, A, there is a single case of marked tendency to favor the sound group. With the lengthened exposure of B, this tendency, as usual, disappears; but returns in C to some extent and two other observers share it. A fourth markedly favors the group without sound. So the experiments of this last table present as marked external evidence as we have for the influence of hearing upon the judgment. These facts are presented in Table XVIII.

TABLE XVIII

	A			B			C		
	44 experiments with each of 4 subjects, 88 with each of 3.			*88 experiments with each of 3 subjects, 44 with each of 3.*			*88 experiments each*		
	Exposure = $\frac{1}{25}$ *sec.*			*Exposure* = $\frac{1}{4}$ *sec.*			*Exposure* = $\frac{1}{25}$ *sec.*		
	Sound	*No-Sound*	*No tendency*	*Sound*	*No-Sound*	*No tendency*	*Sound*	*No-Sound*	*No tendency*
Subjects	1		6			6	3	1	3
Av. % of difference in favor of	27.2		4.1			5.9	12.9	20.4	4.5

It is a further curious fact, well sustained by these same experiments, that where there is some confusion, each of the factors present has a better chance to determine the judgment. The values for both time-error and sound rise higher for the majority in C than in A or B.

VI. THE INFLUENCE OF FACTORS IN THE SAME SENSE-FIELD UPON THE JUDGMENT OF ABSOLUTE NUMBER

The nature of the enquiry that we have been pursuing through so many pages is such that it may be raised exactly as well in the case of absolute as in that of relative number. There appears to be no reason why in this new field the results should not be exactly comparable with those in the old, to be taken indeed as a kind of test for the interpretation to be put upon the old. Without a single exception, unless it were imposed by a technical difficulty, all the earlier factors could be studied with the new purpose. Our practical interest to go to such lengths would depend pretty largely upon the results of first attempts. If wholly confirmatory, these would probably suffice.

The experimental conditions were of the simplest. The 3-8 in.

steel balls of Section III were again pressed into service as objects for the number-judgment. They were thrown loosely into a fixed black frame, 20 cm. square. To avoid suggestive noises, its under-surface was made of a thick piece of felt covered with black cloth; and the whole rested of course on a black-topped table. The exposures were 2 sec. long, timed by watch-ticks. Between experiments the observer held a cardboard screen between him and the objects. When conditions were ready for a new judgment, closing his eyes he lowered the screen, opening his eyes again at the word of command and shutting them at the close of the experiment.

Of course the observers felt that their judgments were for the most part extremely vague. With small numbers they had a greater feeling of confidence. Yet altogether it was surprising with what readiness an absolute number-judgment would spring up in the presence of any given collection whatever within the limits set by the experimental series. Sometimes the observers thought that they made rough calculations on the basis of the filling in a unit of area. So far as this held it would tend to cut off the more astonishing departures from correctness, and it would probably advantage the smaller groups more than the large. Still it was entirely too rough a method to prevent the influence of the factors introduced, as the results will show. There was no time for systematic counting, which, in any case, the observers knew to be forbidden.

The figures in which the observers reported their judgments of absolute number have a value that is chiefly qualitative. The marked inconsistencies and disagreements are our guarantee for this statement. With all the observers there was but the loosest association between group-appearance and number-name. The innumerable variations in internal space-relations were of course responsible. For one observer a particular name probably had a quantitative significance far in excess of its value for another observer in this respect. To one man 100 might have meant about the same as 60, for example, to his neighbor. On the whole they were parsimonious; but Baldwin decidedly not.

A more or less constant influence was exerted on any given judgment by the comparison of the presented group with the traces of the preceding still in mind. The observers felt, however, that the judgment was largely independent of such comparison, and its fluctuations give some credence to this feeling.

The numbers chosen ranged by fives, from 25 to 100. In four cases a number was immediately repeated that rough suggestions

as to the definiteness of the judgment and its dependence upon the actual number might be gained. These were indeed but rough suggestions, since, with certain exceptions to be noticed later, the arrangement was disturbed between times; but they made possible a closer watch upon the flickering of the judgment than could be kept by a mere repetition of the series. In the latter case it might be unstable and yet relatively firm in the other. A standard series is here recorded. Its order was determined by drawing the numbers out of a heap, but the repetitions were inserted arbitrarily.

1. 95	11. 50
2. 25	12. No change
3. 35	13. 60
4. 65	14. 40
5. No change	15. No change
6. 30	16. 70
7. 90	17. 80
8. 85	18. 55
9. 45	19. 75
10. 100	20. No change

1. *Absolute Number under Standard Conditions.*

An indispensable preliminary for the present study is the establishment of a standard. Unless we know something in advance about the characteristics of the judgment of absolute number in relatively simple conditions, we shall be unable to tell what influence, if any, to attribute to the modifying factor in later experiments. Having then decided as to the general conditions under which we will study the problem we must make these the standard conditions of our work; and having discovered the nature of the judgments given under them, measure up to these results in all that is to follow. These standard conditions have already been set forth in the introduction to this section. The results are recorded in Tables XIX and XX.

Turning to these tables we notice at once, as characteristic of all the observers, the following facts: (1) Wide variation from objective correctness. (2) A far wider discrepancy with the larger numbers than with the smaller. Miller does not wholly agree here. His judgments by series show inconstancy, tending at first to follow the rule, but in the last two series to a maximum error near the middle. Certain remarks of this observer suggest that possibly in the latter case reflection as to the convenience of certain actual numbers for manipulation may have had influence. The three earlier series of Hutchison conform to the rule. The remainder, on

TABLE XIX

		Subject=Baldwin			Subject=Miller		
Trials with each number		6	3	3	4	2	3
	Original Numbers	*Standard*	*Scattered*	*Compact*	*Standard*	*Scattered*	*Compact*
The figures recorded	25	1	0	−6	−10	− 6	− 4
are the average of the	30	3	5	−5	−12	− 7	− 6
algebraic sums of other	35	10	7	−3	−14	− 4	− 7
figures that represent the	40	10	13	−8	−15	− 8	−11
difference between the	40	10	8	−8	−14	−10	−12
actual and the esti-	45	10	18	−3	−21	−13	−10
mated number. Frac-	50	17	23	2	−19	−15	−17
tions are replaced by an	50	15	22	2	−17	−15	−14
added unit, if the value	55	27	40	0	−18	− 3	−12
is ½ or over.	60	19	33	5	−20	−23	−17
Baldwin never under-	65	27	53	2	−18	− 3	− 7
estimated the scattered	65	24	57	5	−13	−13	2
group, and only once	70	38	77	0	−19	8	− 8
the standard; but 31	75	31	73	0	−24	10	−20
times the compact. Mil-	75	28	78	−5	−24	8	− 8
ler only once overesti-	80	36	83	−5	−14	5	−18
mated the standard	85	54	85	5	−19	− 8	0
group, and but 6 times	90	54	90	5	−13	8	− 3
the compact; but 13	95	48	73	5	−30	20	−15
times the scattered.	100	61	87	7	−13	10	− 7

the contrary, show no definite progression in tendency. It should be noted here that both Miller and Hutchison were more inclined than the other two observers to rough calculation. The effect of its adoption or of increased practice in it is shown by the disappearance of the characteristics of the earlier series. We have thus in these two cases a doubleness of standard that we must not fail to consider in our later comparisons. (3) There is a pronounced instability of judgment, as shown by the fluctuations for the same number in different series, and especially in successive judgments, of the same in any given series. (4) There is a general tendency to judge in multiples of five. That there should be any splitting of fives, particularly in the large numbers, might be regarded as mere caprice. Not so did it seem to the observers. They were conscious of an apparent absurdity in it where judgments were necessarily so vague; but they insisted that this stood for a kind of qualitative shading

in the perception which threw out the choice of the round numbers just above and below. (5) The number is on the whole underestimated, three observers agreeing in this respect; but the fourth shows a very large and consistent tendency in the opposite direction.

In spite of the manifold special inconstancies and disagreements, these general tendencies are decidedly well-featured in the results. We may say that we have found a kind of standard illusion that will serve us for a guide through our later studies.

2. *The Influence of Distribution.*

TABLE XX

	Subject = Hutchison					*Subject = Olmsted*		
Trials with each number		3		2	4	6	2	3
	Original numbers	*First Standard Series*	*Second Standard Series*	*Mixed Sizes*	*Small Sizes*	*Standard-*	*Mixed Sizes*	*Small Sizes*
For the meaning of these figures see under Table XIX. Hutchison overestimated the standard group only 5 times, never the mixed-size group, and 9 times the small-size group. Olmsted never overestimates at any time.	25	−5	−2	−3	−1	−8	−8	−12
	30	−4	0	−5	0	−11	−14	−16
	35	−6	−5	10	1	−8	−5	−14
	40	−11	−5	−5	−13	−18	−17	−20
	40	−12	−9	−13	−11	−13	−18	−22
	45	−7	−4	−15	−10	−18	−20	−18
	50	−14	−10	−20	−12	−19	−18	−23
	50	−13	−8	−15	−15	−22	−15	−27
	55	−10	−7	−20	−11	−24	−28	−23
	60	−19	−18	−25	−20	−22	−23	−27
	65	−15	−7	−20	−5	−28	−28	−33
	65	−15	−10	−10	−18	−22	−28	−23
	70	−14	−3	−15	−20	−27	−30	−28
	75	−21	−17	−28	−23	−32	−23	−27
	75	−25	−20	−20	−24	−33	−25	−32
	80	−24	−17	−30	−19	−27	−23	−33
	85	−17	−13	−35	−13	−27	−38	−38
	90	−13	−7	−25	−20	−33	−40	−37
	95	−25	−13	−40	−23	−39	−55	−40
	100	−22	−13	−35	−24	−36	−15	−37

The first of the modifying factors to be considered has to do with the arrangement of the objects. Hitherto they had been thrown loosely into the frame. Now in successive studies they were, first, well scattered over the surface and, second, brought together into several compact nuclei. The last arrangement was adopted in preference to that of a single mass as being less open to comparison with pre-

ceding judgments and to judgment on the basis of form and size of group.

The results are shown in Table XIX: (1) The effect of scattering the objects is very markedly to raise the apparent number. Baldwin's preceding overestimations soar still higher; while Miller's former tendency to underestimation is checked to such an extent that 13 overestimations appear. (2) The effect of compacting the objects is just as markedly in the opposite direction. Baldwin gives 31 underestimations, and Miller reverts in a measure to his former type. (3) When similar arrangements were up for study in relative number we found two classes of observers, one favoring the compact, the other the scattered. The present results of Baldwin and Miller put them into the latter class.

3. *The Influence of Complexity of Group-Content.*

This new factor of complexity in the content of the group was realized experimentally by making up the collection out of steel balls of two sizes, 1-8 in. and 3-8 in. The former looked almost infinitesimal beside the latter. The same objective numbers were still maintained and divided between the two sizes except where in so doing a five must be broken. In such a case the extra five went to the larger balls.

The results are found in Table XX. Olmsted shows no definite influence of the new factor. Hutchison, however, shows a very evident decrease in his estimations, when comparison is made with his second standard series. With the earlier series the new results rather closely correspond. That the latter are not simply a vacillating reversion seems fairly clear from this observer's account of his method. The small balls, he says, did not distinctly come in visually. To his judgment of the large he added an amount based on a very insecure estimate of the small. The number of the latter seemed from time to time pretty constant.

This situation corresponds very fully to that in the investigation of the same factor by use of a group of mixed colors, where relative number was in question. (Section II.) The tendency there discovered was to neglect the other colors in favor of one which thus surpassed the others in vividness. There as here the mixed group seemed smaller.

4. *The Influence of Size of Objects.*

A study of this factor was made possible by substituting for the usual objects steel balls of a smaller size, 1-4 in. The results are contained in Table XX. They are not so striking as those obtained

in our study of distribution. Still the influence of this new factor is evident, in the reduction of the apparent number. Olmsted shows this more generally for the smaller numbers. We find it in Hutchison when we compare the new results with the second standard series. This tendency to underestimation increases in the two final series of the present set. At the beginning of these two he remarked that he thought he had been overestimating the group. This tendency of smaller size to reduce apparent number was found true for the majority of observers in our earlier study of relative number.

5. *The Influence of the Length of Exposure.*

I found that in relative number the shorter the look the more marked was the influence of certain factors. Reports of the observers making this seem highly probable happened in this way: When working with the One-Group Apparatus in relative number the shutter of the camera would occasionally stick, leaving a group exposed beyond its usual time. The effect of this upon some but not all the observers was to cause a noticeable shrinking in numerousness. Of those questioned, the only one failing to notice this effect is included among the observers in this new study.

To test this possibility resort was had to the One-Group Apparatus as affording a more satisfactory means for getting different lengths of exposure of small absolute magnitude. Cards were prepared containing a single group of larger area (67×82 mm.) than had been used for relative number. The objects were the usual white circles. Each corner was marked as usual; and, by reason of the number involved, the outline of the area was more regular than had been true in the earlier work. The number of circles on each card varied by steps of two from 16 to 30, giving eight cards in all. The series was arranged irregularly as before, and two of the cards repeated immediately upon their first presentation, making ten experiments in one set. The order of the series follows:

1. 24	6. 28
2. 22	7. 16
3. 26	8. 20
4. No change	9. 30
5. 18	10. No change

Two time-magnitudes were used for comparison, — 1-25 sec. and 1 sec. The latter was managed with bulb exposure. All the experiments with the shorter time were made before those with the longer had been begun. The results are given in Table XXI. So far

as the material is comparable, we may include in our comparison the standard experiments of Tables XIX and XX with 2 sec. exposure.

TABLE XXI

Actual Numbers	*Baldwin* $\frac{1}{25}$ *sec.*	*Baldwin* 1 *sec.*	*Miller* $\frac{1}{25}$ *sec.*	*Miller* 1 *sec.*	*Hutchison* $\frac{1}{25}$ *sec.*	*Hutchison* 1 *sec.*	*Olmsted* $\frac{1}{25}$ *sec.*	*Olmsted* 1 *sec.*
Number of trials with each number	5	6	5	5	5	4	4	4
16	6	9	− 2	− 3	11	− 1	2	− 5
18	8	10	− 4	− 1	12	− 1	− 3	− 5
20	34	19	1	3	19	1	1	1
22	37	17	6	6	14	6	4	− 2
24	49	26	12	5	14	10	5	0
26	70	33	15	8	12	9	5	4
26	79	37	21	9	14	4	9	4
28	93	42	29	11	18	7	8	7
30	106	52	38	12	18	5	19	10
30	103	50	45	11	20	4	19	4

For the meaning of these figures see note under Table XIX.

The outcome may be thus summarized: (1) The apparent number is inversely proportional to the length of exposure. The tables show a perfectly clear progression from 2 sec. to 1-25 sec. All those that formerly underestimated are brought into the opposite class. (2) The results of the earlier experiments are confirmed on the whole with respect to the occurrence of greater errors with the larger numbers. (3) Baldwin's overestimation reaches astonishing heights. (4) These new facts for absolute number are quite in accord with Table XII, where, under the conditions of interpretation laid down, the tendencies were wholly in favor of the shorter look.

The issue of these tentative experiments in absolute number confirms the teaching of our studies in the related field. Absolute number, like relative, has been found largely subject to a modifying influence of certain factors. In the new field, too, distribution has asserted its supremacy among these, and similar effects of shortening exposure have been observed. There has been variation among the observers and some shifting of tendency, both of which point as before to the coöperation of some subjective factor in our results. Indeed the whole situation, as opened by these preliminary studies, indicates a theoretical interpretation that for both fields is at bottom one. So to an attempt to reach such an interpretation the next section will be devoted.

VII. Theoretical Discussion

1. *The Fact of Modification.*

That such an influence upon the judgment of number should have been exercised by the factors considered seems in many cases to receive an adequate account on the principle of association. Our practical experience in the simultaneous variability of number and certain other characteristics of a group of objects has been such as to lead us into illusions when the two no longer vary together. In such a case, when we have no time to count, we are actually led to *see* a group as smaller or larger in accordance with the variations perceived in the associated factor. This interpretation is supported by the fact that on the whole the space-factors were more markedly influential in creating illusions than were any others. For those cases, however, in which the modification was effected by a factor unconnected with number, as color, or the simultaneous stimulation of other senses by irrelevant objects, it appears that the mere occurrence of greater total stimulation during the appearance of one group is sufficient to create illusion, either through failure of the observer to discriminate between the relevant and the irrelevant, or because he is led through fear of disturbance to overemphasize the other group.

2. *The Direction of Modification.*

The foregoing account of the general fact throws no light upon the *direction* of the influence. Why should a given factor make a group seem more numerous and not less? Why should it affect one man in one way and his neighbor in another? Why should it vary with the same man at different times? Appearances no less contradictory than these are what we must face in carrying a theoretical account to completion. The following propositions with appended commentary are offered in satisfaction of these requirements.

a. Differences in vividness among the factors determine differences in number.

Our study of the factor of distribution in Table XVII, where it was possible in a measure to control the vividness, furnishes evidence for this proposition. Introspective reports in other cases confirm this view by showing that the direction of the attention, the popular way of stating our proposition, was the determining feature. This will receive further support in our discussion of the following proposition.

b. If the vivid factor or complex be positive, *i. e.*, associated in

experience with the numerous, or if it be neutral, its group will seem the more numerous. If negative, *i. e.*, associated in experience with the few, its group will seem the less numerous.

The experiments upon the effect of distribution support this proposition, especially as set out in Table XVII. When the vacancies in a given group were made vivid, the other group seemed more numerous; when its filling surpassed in vividness, the judgment was given for it. We have other confirmation in the fact that lengthening the time of exposure reduced the absolute number. Take also this note of one observer on the material in Table II, C:

"I noticed that I had set the open spaces in the outlined group over against the lack of them in the homogeneous, without paying much attention to the nearness together of the spots in the lines of the outlined. Then for a time my attitude was quite vacillating. I found my attention drawn to the nearness together of the spots in part of the outlined group so strongly that if I did not turn it voluntarily to the fact that the other was filled without any large open spaces, I was led to call almost any outlined group the larger. Toward the end of the experiment I got back into my original attitude, in which the outlined group seemed to have its spots hardly more thickly arranged in any part than the homogeneous, and to have also the bare spots and so to be the fewer."

That the vividness of a neutral factor or complex increases the apparent number was suggested by comments of the observers. One observer reported of the material in Table IV: "The greater brightness of red gave it more importance. The natural thing seemed to be to give the red the judgment. The gray fought more for recognition." And again: "The red seems a vitalized space and the dots more omnipresent, also the red lasts longer in memory and is there more vivid, so that often in cases of doubt, where the decisive comparison was made in memory, the red may have been given the vote. Often there was an immediate unanalyzed feeling that if the groups had been both of the same color, the judgment would have been for the gray." In both cases his results showed this tendency. Another observer, whose results agree with the former, found that his eye was directed involuntarily toward the red.

This fact was put to a special test. In the material of Table II, B, a card, in which the pattern group had an appearance strikingly different from the normal, was introduced, the two groups being objectively equal. With three observers the effect was overestimation of this group, and with a fourth, the suppression to equality of a previous

overestimation of the opposite group. This fact, together with the observers' comments, seems to justify the conclusion that the vividness of a neutral factor or complex was the determining condition of the judgment. That the observers did not all show positive results in this experiment may be set down to the difficulty in controlling the subjective conditions of vividness. Of course the space-relations within the new pattern were different from those in the old. The only justification for taking no account of these is the character of the introspections themselves.

It should be said of the red group that beside its vividness it had characters mentioned by other observers that might independently have made its number seem greater. It was called "dazzling," "blurred," and its area seemed increased. In this respect the effect of the color should be discussed as a special case of distribution or object-size.

The vividness tested in this special way seems due to contrast, in the one case with surroundings, in the other case with the expected. Such a judgment is very far removed from the normal bases, rather more so, it would seem, than even those where a group had sound or touch accompaniment; for in the latter case there could be no question about the "moreness"; the only doubt could be about its legitimacy. Of the precise extent to which this cause of vividness has operated throughout our studies, even where spatial differences have been concerned, we cannot be sure. The patterns of the materials in B and C of Table II seem to offer that possibility. That it should enter anywhere opens, indeed, the entire field.

c. The observers fall into the following classes on the basis of the character of the association:

(1) Relatively fixed association,

- (*a*) involving correct adjustment to objects (vividness of relevant factors);
- (*b*) involving incorrect adjustment to objects (vividness of irrevelant factors).

(2) Relatively unstable association.

It will be remembered that in the case of nearly every factor studied under Relative Number, we found three classes of observers, — those favoring one group, those favoring the other group, and a so-called "no-tendency" class. The bases of classification were, first, the relative constancy in the character of the error, and, secondly, its direction. In this third or "no-tendency" class were really lumped off two kinds of observers, not separated at the time because our

special interest did not demand it. Along with those that gave large errors in both directions was a much smaller class that gave a relatively large proportion of correct judgments; but could never claim any one observer all the time. In the new classification of Proposition *c* this mixed composition is recognized by dividing it between (1) (*a*) and (2). The prime condition of correct judgment is asserted to be one in principle with that of the illusion, — namely, vividness, but in this case vividness of relevant factors. Our original "tendency" classes both fall under (1) (*b*).

Proposition *c* is merely an attempt to apply the principles of association and vividness to an organization of our results. The types in question have no hard-and-fast connection with any particular observer; they rather represent a kind of ideal fixation of opposite tendencies playing through all.

3. *The Time-Error.*

So far the time-error has been left without interpretation. The chief facts to be considered were: (*a*) Divergence of error and general trend in favor of last. (*b*) Individual inconsistencies. (*c*) Occasional absence.

We are in a position now to invoke at once the principle of vividness to account for the existence of the error and the vividness of recency to account for the predominant tendency to favor the last of the two groups exposed. In this respect this error may be classed with the effect of red. That is to say, a factor or complex, directly through its vividness and not indirectly through its association with the numerous or the few, draws the judgment after it. Here the content of the group is the effective thing, not the character of the vacancies.

But the observers do not all agree in the direction of the time-error nor are they always consistent. Here we shall get help from a proposition offered in the discussion of the distribution-error in which it is asserted that the group seems the more numerous in which the vacancies are less developed under observation. We have already noted a decided tendency to depend in judging upon the vacancies. Let us suppose that the two groups presented in succession differ with respect to the success of the observer in developing these vacancies. If this be true, that difference may well depend upon the occurrence of maximal attention during the exposure of but one of the groups. The tendency of the majority to overestimate the second group suggests that the attention is likely to be at a maximum when the experiment begins. If, on the other hand, it ripen later, or if

the observer seek to rescue the second group from relative unclearness, then we should have the first group overestimated. The time-error would disappear for those that could attend alike to both. Clearly enough this account is decidedly hypothetical.

4. *The Space-Error.*

Our attempt to reduce this error to one of time in some form was proven a failure. The facts brought out indicate that at the bottom is some subjective factor thus far not isolated. This factor is not a preference going directly with right- or left-handedness because on the surface at least it runs in the observers independent of such asymmetry. A single bit of available introspection would seem, however, to point to some relation of that sort; for one observer, who favored the left, felt that a group on that side gained an importance that was somehow due to the greater absolute value of a weight in the left than in the right hand. Even if this be decisive for him it will still be inapplicable to errors in the opposite direction unless we assume that with variations in bodily energy the emphasis is cast now in one, now in the other, direction, after the analogy of those two types of man to be found in our social experience, for whom respectively mountains are molehills and molehills mountains. Such successive differences in type in a single individual would then find an intelligible account in the shifting tides of that bodily energy. It is to be noted that the observer just quoted once, but only once, made a decisive reversal of his error from left to right.

It may occur to some one that the use of two observers sitting side by side may have given them a preference for one position of the groups. In the first place care was taken that both groups should be as readily seen from one point of view as from the other. Secondly and chiefly, there is no regularity among the observers in this respect.

It is not unlikely that a chance aspect of a particular group develops an emphasis that gives the mechanism of subjective adjustment a particular bent that for a time is relatively independent of the objective situation. Still the fact that there are some cases of persistence in type is rather damaging to this assumption and speaks rather for the earlier one. That one, if true, seems indeed adequate to account for the situation. As an hypothesis it accords with analogous physiological facts; but its weakness lies in imposing the burden of a strong tendency upon asymmetrical differences that may be in comparison relatively slight. Finally, these studies furnish no proof that the bodily condition of an observer of a particular type corresponds to the demands of the hypothesis.

Summary: 1. The estimation of relative number in the visual field is modified by group-area, internal distribution, order, and complexity in group-composition; by the size, form, color, brightness, and complexity of the individual members; and by the character of the environment. It is further modified by factors contributed by the objects through other senses, as in active pressure, special differences in pressure character, active weight, and that complex of muscular and spatial factors arising when a group is observed under the condition of eye-muscle strain. The judgment is also influenced by factors outside the group in the field of touch, but not in that of kinæsthetic impressions.

2. On the whole the most influential factors were those lying in the space-characters of the groups; while those of least moment were contributed by other objects in other fields of sensation. Hearing was very nearly ineffectual.

3. In very many cases the observers fell into three groups, one of no-tendency, and a second and third showing opposite tendencies with respect to the factor investigated.

4. With a majority of observers there is a tendency to underestimation in the judgment of absolute number, though with a single observer the tendency is directly the reverse. Scattering the objects increases, and compacting diminishes, the apparent number. The smaller the size of the objects the fewer, under conditions, do they appear; while heterogeneity in group-composition lessens the number for one observer and has no apparent effect upon the other.

5. The apparent absolute number of objects is inversely proportional to the length of exposure of a group; and in relative number the influence of a factor was on the whole greater for shorter exposures.

6. The marked tendency to a space-error was found to be independent of differences between the groups in the length of look, and of the order in which they were viewed.

7. The distribution-error is grounded in a fundamental tendency to base the judgment of relative number upon the character of the vacancies in a group; though a secondary tendency to depend upon the filling was shown to exist. The subjective factor of vividness, attaching now to one and now to the other of the foregoing factors, determines which shall be operative, though it usually is joined to the first. The ground for the primary tendency may very well be the necessities imposed upon discrimination by the material. The contrast effect between the large black background and the brighter

objects tends to unify the latter, which, to be discriminated as a number, must be split up by an emphasis of the vacancies.

8. The time-error is possibly due to differences in power to dismember the groups exposed in succession in one experiment, while its variations in direction seem adequately accounted for by differences in the time at which attention becomes maximal during the progress of a single test.

9. The ground for these facts of modification is found in the strength of the association between these several factors and the elements that signify number.

10. The basis for the different tendencies found among the observers is the differing vividness among several factors. If the vivid factor is associated with the idea of numerousness, or is in this respect neutral, its group will seem more numerous. If it has been associated with the idea of fewness, its group will seem less numerous. The difference between the two classes of "tendency" and "no-tendency" lies in the fact that for the latter either only correct clues are vivid, or else there is so frequent an alternation in vividness of opposing incorrect clues that through any given series no tendency appears, while for the "tendency" class misleading clues are without shifting in the ascendant.

At the time when these experiments were completed, no work precisely upon this problem had been published. Since then, however, Dr. J. F. Messenger has issued a monograph entitled The Perception of Number (Psych. Rev., Mono. Supp., vol. 5, no. 5), of which certain parts fall within the scope of the present studies. He was concerned with the estimation of absolute number and was primarily interested to discover the nature of the number-judgment. The reader of both articles will find agreement between the results and interpretations here recorded and such part of Messenger's work as has been a common object of study, — viz., the factors of distribution and size.

TIME-ESTIMATION IN ITS RELATIONS TO SEX, AGE, AND PHYSIOLOGICAL RHYTHMS

BY ROBERT M. YERKES AND F. M. URBAN

THE desirability of a statistical study of time-estimation was suggested to us by a note concerning "sex-differences in the sense of time" which was published in Science recently by Prof. Robert MacDougall.[1] By comparing the time-estimates of groups of men and women consisting of fifteen individuals each, MacDougall discovered that for intervals of from one quarter of a minute to a minute and a half, the women exhibited a far stronger tendency to overestimate than did the men, and were at the same time markedly less accurate. The nature and extent of the overestimation discovered by MacDougall are indicated by the results presented in the accompanying table from Science. The numerals, 1, 2, 3, and 4 refer to different fillings of the intervals (listening to reading, marking letters, etc.), the signs + and − to over- and under-estimation respectively.

Period, One Minute.

Sex	1	2	3	4
Men	+29″	+ 1.3″	+22″	− 3.5″
Women	+66	+22.0	+80	+24.0

These apparent sex-differences in time-estimation demand further attention, first, because the number of individuals studied by MacDougall, as he recognized, is too small to establish the fact of the existence of such differences, and, second, because if the differences really do exist they should be studied in their relations to age and the fundamental physiological rhythms.[2]

It seemed probable that further investigation of this subject might reveal some important facts concerning the development of the ability

[1] Science, N. S., vol. 19, pp. 708–709, 1904.

[2] E. Mach (Analyse der Empfindungen, 1900, p. 161) thinks that his physiological time-unit has become larger with age. And he has also noted that the time-sense differs in animals of the same species which differ in size.

to estimate time in the individual, the significance of various conditions for time-estimation, the psychology of sex, and the relations of rhythms to personal affinities, antipathies, and motor capacities.

In this report the results of a statistical study of the sex-differences in time-estimation are discussed, and in later papers we shall present the results of investigations of the relations of time-estimation to age and to individual and sex rhythms, and attempt to work out a convenient and serviceable rhythm-formula. The need of such a formula for expressing individual rhythms is obvious, as is also the need of comparative studies of individual and sex rhythms.

Time Estimation

Name	Place
Age	Date

Order of Tests	Time in Seconds		
	Time of Intervals.	*Male* (17 *years*) No. 1	*Female* (17 *years*) No. 9
1. Idleness	*108″*	*70″*	*120″*
2. Reading	*36*	*30*	*118*
3. Writing	*72*	*36*	*60*
4. Estimating	*18*	*15*	*30*
5. Reading	*108*	*90*	*68*
6. Idleness	*36*	*35*	*60*
7. Writing	*18*	*10*	*10*
8. Estimating	*108*	*100*	*125*
9. Reading	*72*	*100*	*66*
10. Idleness	*72*	*75*	*58*
11. Writing	*36*	*25*	*22*
12. Estimating	*72*	*60*	*60*
13. Reading	*18*	*14*	*15*
14. Writing	*108*	*130*	*59*
15. Estimating	*36*	*30*	*41*
16. Idleness	*18*	*10*	*18*

How did you estimate the interval when you were asked to estimate it as accurately as you could?

n o f e y m i q r s a d r g d e s t k n w e r a x u p x z y o n d f n o d c a e h p m a l
g s r w y t b c k p s o n q a r v q c o m p v r i c p k t o s n q z r l x m i h u v o q g
p p f u t o i c n g s c a r n o t c d a a o b i a r s a d e r w o a i e r g l c r t h f s o r a
e n s i o c r b x g r z b h o w l t s

Number of letters counted	85	88
Pulse-rate	72	81

The experimental data now to be considered were obtained as follows. Record-sheets of the form reproduced above were printed, with blanks for age and name of subject, place, date, for sixteen judgments of time-intervals (numerals 1 to 16), for a statement of the subject's method of estimating time, for the number of letters counted in thirty seconds, and for the pulse-rate. Four intervals were used, 18, 36, 72, and 108 seconds, and for each of these intervals judgments were taken under the four conditions designated on the record-sheet as idleness, reading, writing, and estimating. In the experiments the intervals were not given in order of regular increase or decrease of the length of interval, nor were all the judgments for any one interval taken together, but instead, for the purpose of avoiding the influence of expectation of a particular interval or filling, they were arranged irregularly in the order of column two of the record-sheet. This column, as also columns three and four, which are specimen series of judgments for a male and a female respectively, of course were not printed on the record-sheets which were supplied to the subjects.

The experimental procedure was as follows:

(1) Each subject was given a record-sheet.

(2) The experimenter was provided with a record-sheet on which the time of the intervals numbered from 1 to 16 was given. Care was taken that the subjects should not know the length of the intervals before the experiments.

(3) The beginning of each interval was indicated to the subjects by the word "start" uttered distinctly by the experimenter; the end, by the word "stop."

(4) Before beginning the sixteen tests the experimenter gave a thirty-second interval as a standard of judgment. The experiment then proceeded with only sufficient pause between judgments to allow of the recording of estimates by the subjects.

(5) During the filling called "idleness" the subject did not pay special attention to the estimation of the time, but instead permitted his attention to wander.

(6) During "reading" the experimenter read aloud to the subjects.

(7) During "writing" the subjects wrote from the dictation of the experimenter.

(8) During "estimating" the subjects judged the interval as accurately as they could, by whatever method they chose except the use of a time-piece.

(9) Each subject recorded his judgment of the length of an interval in seconds at the appropriate place on the record-sheet as soon as the interval was ended.

(10) The question following judgment number 16 on the sheet was answered as soon as the sixteen judgments had been recorded.

(11) The number of letters counted in thirty seconds was determined by the use of the lines of letters at the bottom of the sheet. The subjects began at the left of each line and counted singly as many letters as they could between the "start" and "stop" signals of the experimenter. They then marked the last letter counted and immediately recorded, in the place provided on the record-sheet, the number of letters counted.

(12) The pulse was counted by the experimenter immediately after the experiment when possible and the rate recorded on the sheet.

(13) The experimenter avoided delays, interruptions, or other irregularities in the course of the series of experiments.

The materials for our discussion of the sex-differences in time-estimation consist of the judgments of 251 males and 274 females. The majority of the males were students in Harvard College, the majority of the females, in Radcliffe and Smith Colleges. The remainder of the records were obtained in Ohio State University, Pomona College, and West Chester State Normal School. The authors gratefully acknowledge their indebtedness for assistance in the obtaining of records to Professors A. H. Pierce, T. H. Haines, W. H. Scott, D. R. Major, A. M. Smith, H. A. Miller, and B. T. Baldwin. The males ranged in age from 17 to 23 years, the females from 17 to 20. The total number of judgments, the distribution of which among the various ages is shown in Table 1, is 4014 for the males, 4375 for the females.

Despite the fact that our experiments are open to the criticisms of all work done under variable conditions and by different experimenters, it cannot be doubted that the results indicate certain sex-differences in time-estimation which suggest additional problems. For the present we refrain from interpretations for the most part and state merely the statistical results of the investigation.

Previous studies of the "time-sense" and the conditions which influence time-estimation suggested to us the desirability of examining our data with reference to (1) sex-differences in estimates of intervals, (2) age-differences, (3) the influence of different fillings, and (4) differences dependent upon the length of the interval. The results have been studied, therefore, with reference to the significance of sex, age, filling, and length of interval, but as no marked age-differences appeared, the detailed tables which were constructed to exhibit the results for the subjects of each year of age have not been printed.

In all the tables the results for males and females are presented

separately. The judgments for the sixteen intervals are arranged with reference to the length of the interval, not in the order in which they were taken; all the 18″ intervals, for example, are grouped (Table 2). The letters I, E, R, W, refer to the fillings of the intervals.

TABLE 1

NUMBER OF SUBJECTS, AGE, SEX, AND NUMBER OF JUDGMENTS

Males

Age	*No. of subjects*	*No. of judgments*
17 yrs.	16	256
18	27	432
19	40	639
20	67	1071
21	50	800
22	35	560
23	16	256
Totals	251	4014

Interval	*No. of judgments*
18″	1004
36″	1003
72″	1003
108″	1004
Total	4014

Females

Age	*No. of Subjects*	*No. of judgments*
17 yrs.	73	1160
18	57	911
19	64	1024
20	80	1280
Totals	274	4375

Interval	*No. of judgments*
18″	1092
36″	1094
72″	1096
108″	1093
Total	4375

A general survey of the individual records, all of which for any one year and sex were tabulated, for convenience of examination, on a single large sheet of coördinate paper, showed that the judgments vary within a wide range and are very inexact. Table 2 exhibits the num-

ber of correct judgments for each sex, interval, and filling. Of the 4014 male judgments only 96 (2.39 %) were correct; of the 4375 female judgments only 46 (1.05 %) were correct. The number of correct judgments decreases as the length of the interval increases. For the 18-second intervals there were 7.37 % for the males, 2.48 % for the females, while for the 108-second intervals there were only 0.10 % and 0.37 % respectively.

TABLE 2

FREQUENCY OF OCCURRENCE OF CORRECT JUDGMENTS

	18″				36″				72″				108″					
	I	E	R	W	I	E	R	W	I	E	R	W	I	E	R	W	Σ	%
Males																		
	29	26	11	8	5	6	3	0	3	3	0	1	0	1	0	0	96	2.39
Totals	74 = 7.37 %				14 = 1.40 %				7 = 0.70 %				1 = 0.10 %				96	2.39
Females																		
	7	15	1	4	2	4	1	0	2	5	0	1	2	1	0	1	46	1.05
Totals	27 = 2.48 %				7 = 0.62 %				8 = 0.73 %				4 = 0.37 %				46	1.05

List of abbreviations which occur in the tables.

Σ always designates the sum of the results of the column which it heads.

I, E, R, W refer respectively to the intervals of idleness, estimating, reading, and writing.

The % sign refers to the value of the result in question in terms of the total number of judgments.

C refers to the results of the letter-counting test.

The male judgments for the 108-second intervals range from 11 to 300 seconds. If random guesses be made within these limits the probability of the occurrence of right guesses (108″) would be 1 in 290; therefore among 1004 guesses (the number of male judgments for 108-second intervals) 3.5 would be right. In the experiment only one judgment of the 1004 was correct. For the other intervals, with the exception of 18 seconds, the number of correct judgments is only slightly greater than random guessing would have given. Both males and females, however, show considerably more correct judgments for 18-second intervals than the number of probable right guesses. Within the range of the male judgments and for their number 16.9 right guesses might be expected, for the females 10.9. In contrast with these numbers the experiments furnished 74 and 27 correct judgments respectively.

It is noteworthy that for those intervals which are most frequently

correctly judged, not only is the number of correct judgments greater for the males than for the females (the ratio of the percentages is about 3 to 1), but the ratio of the number of correct judgments to the probable number of right guesses is also greater for the males.

The female judgments vary within a wider range and are less often correct than the male. For the latter the total number of correct judgments is more than twice that for the former.

Another interesting fact concerning the judgments of the time-intervals is that certain numerals occur in the last place of a judgment more frequently than we should expect if their occurrence depended on random guessing. Tables 3 and 4 exhibit the results of an analysis of the data made for the purpose of studying this fact. In Table 3 the frequency of occurrence of the digits 0, 1, 2, 3, etc., in the last place of the male judgments is given for each filling under the four intervals. For example, the digit 0 occurred in the last place of the male judgments for the interval reading 36 seconds 98 times, as we learn by referring to the first line and third row of the second column of Table 3.

Examination of Tables 3 and 4 shows at once the marked preference of the subjects for 0 and 5. The percentage of male judgments which end in 0 is 41.50; of female 58.51. Similarly the percentages of occurrence of the digit 5 for the males is 24.41, and for the females 23.11. Only two of the other digits (2 and 8) occur with a frequency of over 5 %.

Among the 4014 male judgments 0 occurred as a final digit 1666 times, 5, 980 times. Among the 4375 female judgments 0 occurred 2560 times, 5, 1011 times. In the male judgments 0 occurred about four times as often as it would in random guessing; in the female, almost six times as often.

Comparison of Tables 3 and 4 indicates that the occurrence of 0 is 17.10 % greater for the females, while that of 5 is 1.30 % greater for the males. The sum of the percentages of occurrence of 0 and 5 for the males is 65.91, therefore the probability that a male judgment ends in one of these digits is almost twice that in favor of any other digit. For the females the sum of the same percentages is 81.62, and the probability of occurrence of 0 or 5 is therefore more than four times that of the other eight digits.

Tables 3 and 4 show that even numbers occur more frequently than uneven as final digits. Of the total number of judgments 2461 (3063) [1] end with even digits and 1553 (1312) with uneven.

[1] The unbracketed number is that for the males, the bracketed that for the females.

TABLE 3. FREQUENCY OF OCCURRENCE OF THE NUMERALS (0 TO 9) AS FINAL DIGIT OF THE JUDGMENTS
COMBINED RESULTS FOR ALL MALES

	18"				36"				72"				108"						
	I	E	R	W	I	E	R	W	I	E	R	W	I	E	R	W	Σ	%	C
0	70	57	72	76	131	77	98	90	124	74	120	125	151	121	127	153	1666	41.504	21.92
1	5	11	9	9	10	9	8	7	1	18	5	2	4	11	10	2	121	3.014	7.57
2	20	15	21	18	5	19	16	15	11	20	5	14	9	12	9	8	217	5.406	13.15
3	9	17	14	10	8	11	10	8	6	14	12	4	3	12	14	3	155	3.861	7.17
4	8	12	14	9	3	14	12	3	4	14	8	7	6	18	9	3	144	3.587	7.57
5	67	58	56	63	62	54	60	85	72	59	62	73	54	45	52	58	980	24.414	10.36
6	13	17	12	13	9	14	11	6	10	9	7	7	8	7	6	4	153	3.812	8.76
7	15	22	14	12	6	15	11	10	5	14	13	4	4	7	10	8	170	4.235	7.17
8	36	30	29	31	15	19	16	20	11	17	11	11	8	13	5	9	281	7.004	8.76
9	8	12	10	10	2	18	9	7	7	12	8	3	4	5	9	3	127	3.164	7.57

TABLE 4. FREQUENCY OF OCCURRENCE OF THE NUMERALS (0 TO 9) AS FINAL DIGIT OF THE JUDGMENTS
COMBINED RESULTS FOR ALL FEMALES

	18″				36″				72″				108″						
	I	E	R	W	I	E	R	W	I	E	R	W	I	E	R	W	Σ	%	C
0	120	117	106	126	169	124	182	151	176	145	175	194	226	167	196	186	2560	58.515	26.14
1	4	8	3	2	6	7	1	3	2	3	2	1	1	5	2	0	50	1.143	9.09
2	17	18	12	13	9	22	7	11	7	11	3	4	4	11	6	3	158	3.611	5.30
3	4	9	9	11	5	9	3	7	6	15	10	4	3	7	4	7	113	2.583	5.68
4	2	11	13	10	1	6	4	2	9	14	3	1	2	4	2	2	86	1.966	11.36
5	89	63	91	85	62	61	60	79	56	55	65	63	32	44	49	57	1011	23.109	14.39
6	9	12	11	6	5	13	7	5	4	6	3	3	0	9	3	1	97	2.217	6.44
7	5	5	7	7	4	9	5	3	2	7	3	3	0	5	2	5	72	1.646	6.44
8	16	22	17	12	10	13	5	10	10	9	6	1	5	11	8	7	162	3.703	10.61
9	5	9	5	1	3	9	0	2	2	9	4	0	1	10	2	4	66	1.508	4.55

In order that the probability of the occurrence of even and uneven numbers may be calculated, those judgments which end in 0 and 5 must be subtracted from the total number of judgments, for the occurrence of these two digits is apparently due to a constant influence. The problem may be formulated thus. First, what is the probability that a judgment is determined by the constant influence in favor of 0 and 5? Second, what is the probability of even and uneven numbers, when the influence in favor of 0 and 5 is eliminated? The calculated probability of 0 or 5 is 0.65919 (0.81622) and therefore the probability that a given judgment is not determined by this influence is 0.34081 (0.18378). The probable limits of these numbers are 0.00505 (0.00395).

After the subtraction of those judgments which end in 0 or 5, there remain 1368 (804), of which 795 (503) are even and 573 (301) uneven. The probability of an even number is 0.58115 (0.62562) and the inverse probability of an uneven number is 0.41885 (0.37438). The probable limits of these numbers are 0.00900 (0.01027). There are therefore even chances that the percentage of occurrence of even numbers is between the limits 57.215 and 59.015 (61.535 and 63.589), or outside these limits.[1]

Statistical studies have already proved that in random guessing even numbers occur somewhat more frequently than uneven. It is therefore worthy of notice that in these results the frequencies of even numbers are not uniformly greater than that of uneven; for with the exception of the digit 6 in the female judgments, the digits next to 0 and 5, *i. e.*, 9 and 1, 4 and 6, occur with least frequency.

In the case of the number of letters counted in a half minute, also, it appears (see last column (C) of Tables 3 and 4) that 0 and 5 occur more frequently in the last place than chance would lead us to expect. In contrast with the results for the time-judgments, in the same tables, the percentages of occurrence of the various digits in counting present less marked differences. For the males 3 and 7 occur least frequently, for the females 2 and 9.

To sum up the results of our examination of the materials with reference to the occurrence of digits in the final place of the judgments, the

[1] It may be asked in connection with the above statistics, what is the probability that even and uneven digits have unequal chances of occurrence? The analytic treatment of this problem indicates that the probability is $\frac{1}{2}[1+\Phi(4.25)] = 0.9999999891$ for the males and $\frac{1}{2}[1+\Phi(15.93)]$ for the females. It is therefore almost certain, theoretically, that even and uneven numbers have not the same frequency. See Czuber, Wahrscheinlichkeitseechnung, 1903, pp. 158–161. The constants of this problem are in Czuber's denotation, $a=52.308$ and $b=222$.

order of decreasing frequency of the various digits is 0, 5, 8, and 2. Of the others 3 and 7 occur more frequently than 4 and 6, with one exception. The statement that even numbers in general occur more frequently than odd must be modified by the statement that in these results the digits next to 0 and 5, namely, 9, 1, 4, and 6 occur with least frequency. These statements hold for both males and females, but for the latter the frequency of occurrence of 0 is far greater than for the males.

These results clearly indicate that the judgments are not random guesses. In seeking further for some explanation of the surprising frequency of occurrence of judgments which end in 0 or 5, we discovered that certain numbers occur very frequently, namely, the multiples of 15, 30, and 60. In order to exhibit this tendency quantitatively Tables 5 and 6 have been constructed.

In these tables will be found tabulated the number of times 15 and multiples of it which are not also multiples of 30 or 60 occur for any given interval and filling. Likewise are tabulated the frequencies of occurrence of 30 and multiples of it which are not multiples of 60, and finally, of 60 and its multiples. The numbers as they occurred in the three categories run as follows:

15	30	60
45	90	120
75	150	180
105	210	240
135	270	300
165	330	360
195	390	420

Fifteen and its multiples, as given above, are arranged in one division of the tables, thirty and sixty each in its own separate division. The line at the bottom of the tables marked Σ gives the frequency of occurrence of these three groups of numbers for all the subjects and for each interval and filling.

As is shown by the percentage of frequency columns of the tables, in no instance do the multiples of 15 constitute less than 19.52 % of the male judgments and 23.81 % of the female judgments. The lowest frequency for any of the four intervals is 20.32 % of the total number of judgments. The maximum frequency for the males (43.03 %) and for the females (56.57 %) is for the interval idleness 108 seconds. That the male and female maxima should fall on the same interval is interesting.

If no influence worked in favor of the multiples of 15 in these experiments only one judgment in thirty would be 15 (3.33 %) and only one

TABLE 5

FREQUENCY OF OCCURRENCE OF 15, 30, 60, AND THEIR MULTIPLES

Males

		15	30	60	Σ	%	*Average*
18″	I	48	5	0	53	21.12	
	E	42	9	0	51	20.32	20.32
	R	43	8	0	51	20.32	
	W	45	4	0	49	19.52	
36″	I	29	60	7	96	38.25	
	E	17	41	5	63	25.20	29.57
	R	22	41	6	69	27.41	
	W	36	28	5	69	27.41	
72″	I	29	18	46	93	37.05	
	E	25	7	34	66	26.29	34.70
	R	28	26	33	87	34.66	
	W	28	42	32	102	40.80	
108″	I	25	31	52	108	43.03	
	E	16	23	20	59	23.51	36.16
	R	22	30	39	91	36.25	
	W	25	37	43	105	41.83	
Σ		480	410	322	1212		
%		11.96	10.21	8.02	30.12		

TABLE 6

FREQUENCY OF OCCURRENCE OF 15, 30, 60, AND THEIR MULTIPLES

Females

		15	30	60	Σ	%	*Average*
18″	I	58	30	0	88	32.47	
	E	30	35	8	73	26.64	28.48
	R	56	26	3	85	31.02	
	W	41	24	0	65	23.81	
36″	I	31	55	39	125	45.62	
	E	21	36	17	74	27.21	38.86
	R	31	63	33	127	46.35	
	W	41	45	13	99	36.26	
72″	I	29	28	60	117	42.70	
	E	17	18	52	87	31.75	41.60
	R	26	37	52	115	41.97	
	W	30	51	56	137	50.00	
108″	I	12	45	98	155	56.57	
	E	21	27	58	106	38.83	47.83
	R	23	36	84	143	52.19	
	W	24	31	64	119	43.75	
Σ		491	587	637	1715		
%		11.22	13.42	14.56	39.22		

in sixty, 30 or 60 (1.67 %). For the probability of the occurrence of 15, 30, and 60 is $\frac{1}{60} + \frac{1}{60} + \frac{1}{60} = \frac{1}{15}$, as is obvious from the fact that among sixty consecutive numbers (1 to 60) there are four which are multiples of 15. According to probability we should expect multiples of 15, 30, and 60 to occur 268 times among the 4014 male judgments and 292 times among the 4375 female judgments. As a matter of fact there are 1212 such judgments for the males, 1715 for the females. The probability that a male judgment is a multiple of 15, 30, or 60 is 0.3012 (probable error 0.0049); for a female judgment the probability is 0.39199 (probable error 0.0050).

These statistics indicate that the subjects are constantly and strongly influenced in favor of judgments which are simple fractions of a minute. Closer inspection of the tables gives some suggestion of the nature of this influence.

Comparison of the four intervals (Tables 5 and 6) with respect to the occurrence of simple fractions of a minute shows that the frequency of such numbers increases rapidly as the length of the interval increases. The various percentages of frequency for males and females and for the four intervals are again presented here for convenience of comparison.

	18″	36″	72″	108″
Males	20.32 %	29.57 %	34.70 %	36.16 %
Females	28.48	38.86	41.60	47.83

As is obvious from these figures both frequency and rate of increase are far higher for the females than for the males.

Examination of the percentages (totals) at the bottom of Tables 5 and 6 reveals another remarkable sex-difference; for the frequency of occurrence of 15 and its multiples regularly decreases for males from the 15 to the 60 class, whereas for females it regularly increases.

Undoubtedly the time-judgments of these experiments were strongly influenced by thought of the conventional time-unit, the minute, for in all quantitative work there are errors in favor of the standard of measurement and simple fractions thereof. In the present instance this tendency to favor the unit was strengthened, perhaps, by the giving of a half-minute interval as a standard for comparison at the beginning of the tests.

Two explanations of the sex-differences above mentioned are suggested by our study of the data. One is the fact that the females are less exact than the males; the other that they generally overestimate the intervals, whereas the males often underestimate them. One's estimate of an interval is determined partly by confidence of accuracy.

The longer an interval the less we feel able to estimate it accurately, and, as a consequence, the more frequently it is judged as the same as the time-unit or a simple fraction of that unit. The females are less exact in their estimates than the males, and less exact for long than for short intervals, and as an accompaniment of their inexactitude we find the frequent occurrence of multiples of 15, 30, and 60.

But confidence of ability to estimate accurately must be considered in connection with the fact which suggests our second explanation, namely, that the female estimates are higher than the male. Tables 7 and 8 show that the females almost invariably overestimate the intervals rather largely, while the males sometimes underestimate considerably. The range of the male judgments is from 1 to 300, of the female from 1 to 400. Obviously the chance of occurrence of 15, 30, 60, and their multiples varies with the range. The greater the range the greater the probable frequency of 30 and 60 in comparison with 15. In random guessing the probabilities of the occurrence of 15, 30, and 60 for long and short intervals is the same, but our results show that this is not true in the case of these time-estimation judgments. It seems possible, therefore, that the sex-differences referred to are due to the fact that the intervals seem longer to the females, and that, therefore, a feeling of greater inexactitude than would be felt for shorter intervals leads to the choice of simple fractions of a minute more frequently than in the male judgments and more frequently for the long than for the short intervals.

It is of interest in this connection to note that the length of a second is usually underestimated by females, overestimated by males. The average number of seconds counted in half a minute by twenty men and twenty women was as follows:

Men. M. 30.4, M.V. 8.7, R.V. 34.94. *Women.* M. 38.9, M.V. 10.6, R.V. 36.70

These figures would seem to indicate that the overestimation of the intervals of these experiments by the females is due to the use of a time-unit which is shorter than that of the males (although presumably of the same length).

We cannot with certainty say whether inaccuracy of judgment stands in the relation of condition or consequence of the occurrence of simple fractions of a minute, but it would appear that the female tendency to overestimate is responsible for the sex-differences already noted. For whatever be the facts concerning longer intervals the second as judged by the female is considerably shorter than that of the male.

Since a complicated periodicity of frequency in the distribution of

the judgments is exhibited in the results of Tables 3–6 it is obvious that the distribution-curve will have a tertiary mode for each number ending in 0 or 5, a secondary mode for 15, 30, 60, and their multiples, and a primary mode which may or may not coincide with one of the secondary or tertiary modes. Extreme irregularity is characteristic of the distribution-curve. Different groups of judgments, as, for example, those for the two sexes, those for the different intervals, etc., give somewhat different forms of distribution, for the frequency of occurrence of 0 and 5, as well as of the multiples of 15, is variable.

These facts are important in connection with the selection of an interval for the construction of the distribution-curves, in that they indicate how large the interval or class of the distribution-curves and tables should be.

It is clear from the results of Tables 3 and 4 that the smallest interval which can be of value is 10 seconds, for a smaller interval would necessarily exhibit irregularities due to the greater frequency of 0 than of 5. The question is whether the interval can be so enlarged, without the loss of all details of the nature of the distribution, that every class will represent the influence of the same conditions. For this purpose only three intervals are possible: 10, 30, and 60 seconds. Of these 30 and 60 are undesirable because the interval 60 gives classes which are so large that all details of the distribution are lost, while 30 exhibits only a few details without doing away with the periodicity due to the preference for multiples of 60.

The further question remains, with which digit should the interval end, in order that uniformity of conditions for the various classes may be gained? Theoretically there are ten possibilities, but of these all except two, 0 and 5, are excluded by reason of the unequal frequency of the various digits already discussed. In favor of 0 is the fact that all the classes thus formed are of equal size, *i. e.* 1–10, 11–20, etc., whereas for 5 the first class would differ from the others in being only half as large, 1–5. This, however, is only a slight disadvantage, for there are very few judgments which fall in this class. On the other hand, since 0 is the final digit of most frequent occurrence, classes ending in 5 have the advantage of placing the value of greatest weight in the middle. On the whole it seemed desirable to arrange the judgments in 10 second classes, beginning with the class 1–10. But for purposes of comparison the male judgments have been distributed in classes of 10 seconds, which end in 5, 1–5, 6–15, 16–25, etc.[1]

[1] The judgments might be grouped in 10 second classes beginning with the lowest number in the experiments, but this would have the disadvantage of ren-

TABLE 7

DISTRIBUTION OF MALE JUDGMENTS IN 10″ CLASSES

Classes	18″				36″				72″				108″				C
	I	E	R	W	I	E	R	W	I	E	R	W	I	E	R	W	
1 — 10″	31	16	69	136	3	1	4	18	1	1	1	4	0	0	0	0	0
11 — 20	174	179	142	108	40	34	45	97	4	2	5	23	1	2	1	7	0
21 — 30	34	47	35	7	99	106	98	82	15	9	26	65	5	0	6	23	0
31 — 40	7	7	3		56	69	69	28	25	16	34	47	10	2	6	31	1
41 — 50	4	2	2		37	23	22	18	41	43	54	33	20	9	19	28	0
51 — 60	1				9	13	9	6	64	68	53	35	31	17	27	32	7
61 — 70					5	3	0	0	37	42	28	18	27	26	22	19	8
71 — 80					0	1	3	1	23	34	16	9	33	34	35	20	34
81 — 90					1		1	1	15	15	15	8	30	40	43	29	58
91 — 100					0				9	8	7	1	23	35	25	17	65
101 — 110					0				2	6	3	3	10	26	17	6	33
111 — 120					0				8	5	5	2	29	16	17	16	29
121 — 130					0				3	1	2	0	5	15	7	8	10
131 — 140					0				1	1	1	1	6	9	5	0	4
141 — 150					1				0		1	0	5	5	8	4	1
151 — 160									1			0	2	3	2	2	1
161 — 170									1			0	2	1	0	2	
171 — 180									1			1	5	4	5	2	
181 — 190													0	1	2	1	
191 — 200													3	2	2	2	
201 — 210													0	1	1	0	
211 — 220													0	0	0	0	
221 — 230													1	1	0	0	
231 — 240													2	0	1	0	
241 — 250													1	1		1	
251 — 260														0		0	
261 — 270														0		0	
271 — 280														0		0	
281 — 290														0		0	
291 — 300														1		1	
Totals	251	251	251	251	251	250	251	251	251	251	251	250	251	251	251	251	251

As a result of these groupings of the male judgments it appeared that the former method gives a far more regular distribution than the latter. In view of this result and the above considerations, Tables 7 and 8 were constructed by the use of 10-second classes, beginning with 1–10. In these tables (column C) the distribution of the letter-counting results has been included for convenience of comparison of the two kinds of judgments as to form of distribution.

As instances of the general form of distribution of the judgments the curves have been plotted for letter-counting, Fig. 1 *A*. (Males ——, Females, . . . ,) for idleness 36 seconds, Fig. 1 *B*, and for idleness 108 seconds, Fig. 2. The distribution of the letter-counting judgments in

dering the distribution tables or curves for different groups of judgments incomparable.

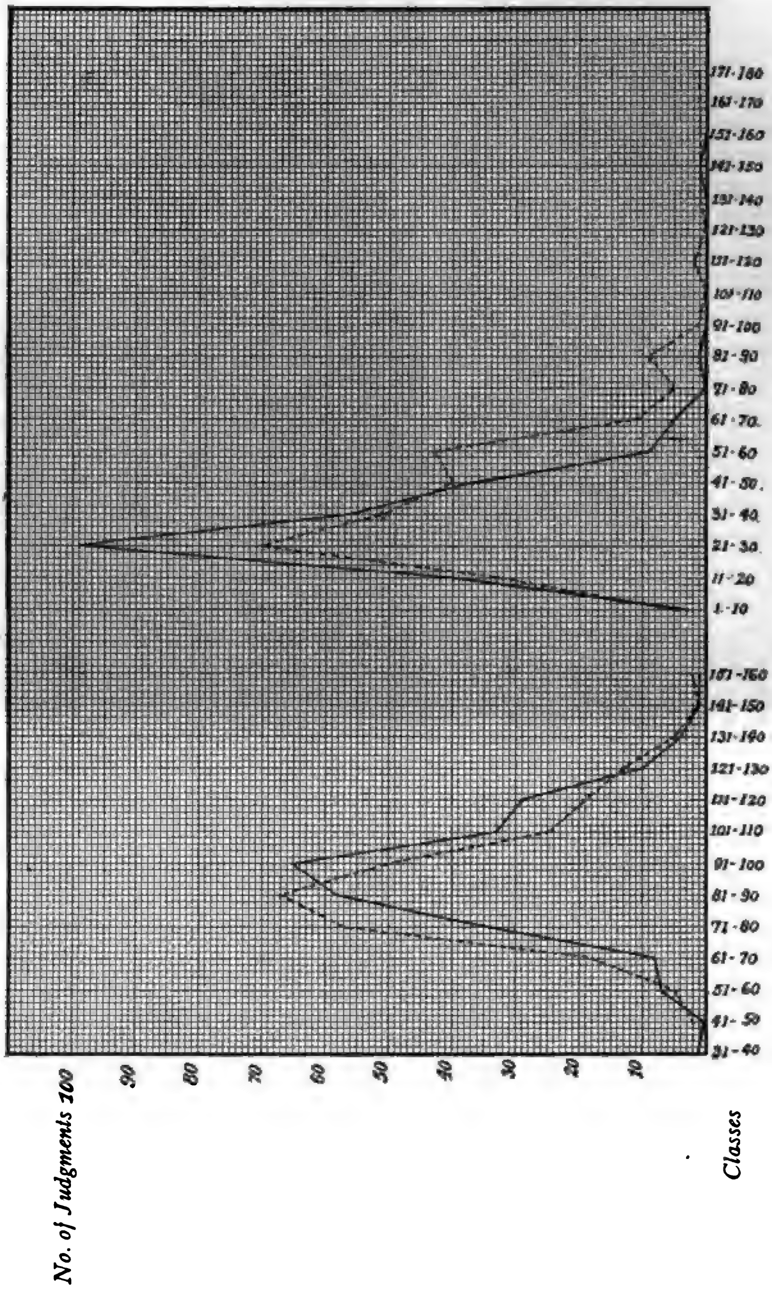


Fig. 1*A*. Distribution of the Results of the Letter-Counting Test. Males —— Females . . . .

Fig. 1*B*. Distribution of the Time-Estimation Judgments for the Interval Idleness 36″. Males ——, Females . . .

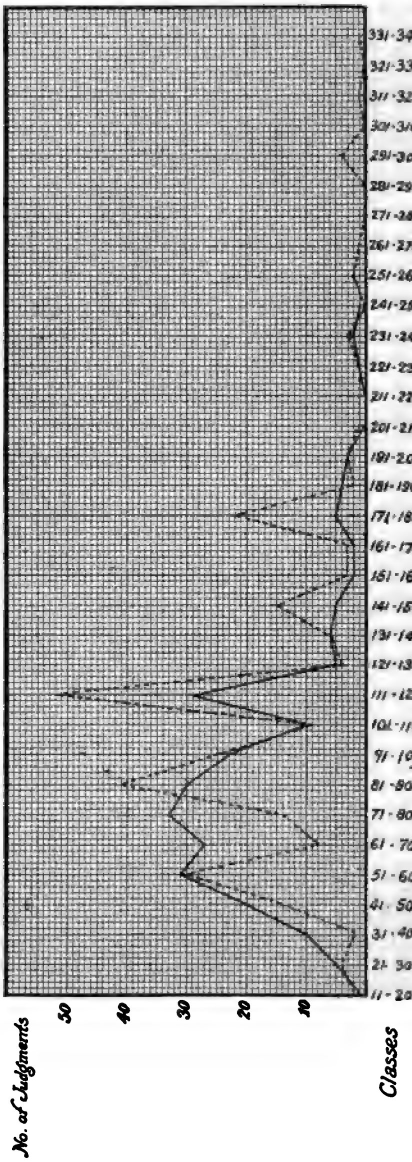


Fig. 2. Distribution of the Time-Estimation Judgments for the Interval Idleness 108″.
Males ——, Females . . . .

classes of 10 is very regular in comparison with that of the several time-estimation judgments. For idleness 36 seconds there are several fairly distinct modes, and for idleness 108 seconds the modes are still more numerous and more marked.

TABLE 8

DISTRIBUTION OF FEMALE JUDGMENTS IN 10″ CLASSES

	18″				36″				72″				108″				
Classes	I	E	R	W	I	E	R	W	I	E	R	W	I	E	R	W	C
1 — 10	72	20	88	133	5	6	5	33	2	0	1	6	0	1	0	2	0
11 — 20	114	119	109	94	33	29	40	83	7	5	15	25	2	2	7	10	0
21 — 30	50	87	54	29	70	57	73	74	16	13	25	52	4	1	3	27	0
31 — 40	17	17	10	6	51	87	53	30	24	21	24	30	2	1	11	14	0
41 — 50	10	14	7	4	40	35	39	20	35	18	37	32	14	6	17	34	2
51 — 60	2	10	3	1	43	30	40	14	48	44	47	53	30	18	35	36	5
61 — 70	1	3	0	3	11	7	5	2	28	50	20	21	8	11	23	22	18
71 — 80	3	2	1	2	5	8	2	10	29	32	23	14	14	21	20	14	57
81 — 90	0	0	1	1	9	3	6	2	27	29	28	14	41	35	35	19	67
91 — 100	0	2	0		1	4	2	1	16	11	15	5	25	35	17	11	50
101 — 110	0		0		0	1	1	1	4	7	6	0	9	24	7	6	25
111 — 120	0		0		2	3	6	3	17	20	10	8	52	38	33	16	19
121 — 130	2		0		0	1	0		3	4	3	3	4	15	6	8	13
131 — 140			0		1	1	0		1	2	1	4	6	9	6	6	5
141 — 150			0		1	0	1		5	4	4	2	15	13	16	7	1
151 — 160			1		1	0	0		0	4	2	0	3	7	2	2	2
161 — 170					0	0	0		1	1	0	0	3	2	0	1	
171 — 180					1	0	0		7	4	7	4	22	14	16	22	
181 — 190						0	0		1	0	1	0	2	2	3	0	
191 — 200						0	0		1	1	3	0	3	5	6	6	
201 — 210						1	0		0	0	0	0	1	0	1	0	
211 — 220							0		0	1	0	0	1	2	0	0	
221 — 230							0		0	1	0	0	1	0	6	2	
231 — 240							0		2	1	0	1	3	3	2	4	
241 — 250							0			1	0		0	3	0	0	
251 — 260							0				0		2	0	0	1	
261 — 270							0				0		1	0	0	0	
271 — 280							0				0		0	0	0	0	
281 — 290							0				0		0	0	0	0	
291 — 300							1				2		4	2	1	1	
301 — 310													0	0	0	0	
311 — 320													1	0	0	0	
321 — 330													0	0	0	0	
331 — 340													1	0	0	1	
341 — 350														0	0		
351 — 360														1	1		
361 — 370														0			
371 — 380														0			
381 — 390														0			
391 — 400														2			
Totals	271	274	274	273	274	273	274	273	274	274	274	274	274	273	274	272	264

Tables 7 and 8 show that the range of the judgments increases with the length of the interval judged, and that the modal class is always

much nearer the lower than the upper limit. Asymmetry is characteristic of the distribution of organic data, and in certain instances, as for example writing 18 seconds, males, the choice of a 10-second class interval results in extreme asymmetry, and one is reminded of the tables which Fechner gave as examples of his logarithmic method in statistics.[1]

It is not to be expected that a method of grouping should be found which will give regularity of distribution throughout, but it is important that there should be regularity about the mode. In the table of distribution for the males (Table 7) all the intervals from idleness 18 seconds to reading 72 seconds are regular.[2] The remaining intervals, with the exception of estimating 108 seconds, are irregular.

Trial proves that for these intervals increase of the size of the class to 30 seconds is sufficient to give regular distributions, as is obvious from Table 9. Grouping by 30-second classes gives regularity for most of the female judgments, but for idleness 108 seconds and writing 108 seconds there are still slight irregularities, as Table 10 indicates.

Tables 7 and 8 show that the distribution is far less regular for the females than for the males. The fact that it becomes regular when the class interval is increased to 30 seconds suggests that the irregularities of distribution which appear in the tables are due to those influences which favor simple fractions of a minute and not to the small number of judgments.

Having now noted certain important characteristics of the time-estimation judgments and the nature of their distribution, we may examine the arithmetical means and other statistical quantities which have been determined for our data. Those quantities which have been determined for the several ages, intervals, and fillings as well as for the sexes are: (1) The Mean (*M.* in tables), (2) the average variability (*M. V.*), (3) the positive variability (+*V.*), (4) the number of judgments with positive variation (No. + *V.*), (5) the negative variability (— *V.*), (6) the number of judgments with negative variation (No. — *V.*), (7) the relative variability (*R. V.*) $= \frac{M+V}{M} \times 100$.

Since the sums of the positive and the negative variations are equal,

[1] G. Th. Fechner, Collectivmaasslehre, pp. 338–346, 1897. Compare the asymmetry of writing 18 seconds for both males and females with Fechner's Table III, p. 442.

[2] The distribution may be called regular if the classes increase to a maximum, the mode, and then decrease without interruption. (See Fechner, Collectivmaasslehre, pp. 121–122.)

it is possible to make certain of the accuracy of the means and average variabilities by comparison of the $+V$. and $-V$. As this was done in all cases we feel confident of the reliability of the statistical quantities presented in the tables.

In Table 11 have been arranged the various quantities as determined for the males and females for each interval. The values given in this table are averages of the determinations made for the several ages separately.

Table 9

Distribution of Certain Male Judgments in 30″ Classes

Classes.	W. 72″	I. 108″	R. 108″	W. 108″
1 — 30	92	6	7	30
31 — 60	115	61	52	91
61 — 90	35	90	100	68
91 — 120	6	62	59	39
121 — 150	1	16	20	12
151 — 180	1	9	7	6
181 — 210		3	5	3
211 — 240		3	1	0
241 — 270		1		1
271 — 300				1
	250	251	251	251

Table 10

Distribution of Female Judgments in 30″ Classes

	18″				36″				72″				108″			
Classes	I	E	R	W	I	E	R	W	I	E	R	W	I	E	R	W
1 — 30	236	226	251	256	108	92	118	190	25	18	41	83	6	4	10	39
31 — 60	29	41	20	11	134	152	132	64	107	83	108	115	46	25	63	84
61 — 90	4	5	2	6	25	18	13	14	84	111	71	49	63	67	78	55
91 — 120	0	2	0		3	8	9	5	37	38	31	13	86	97	57	33
121 — 150	2		0		2	2	1		9	10	8	9	25	37	28	21
151 — 180			1		2	0	0		8	9	9	4	28	23	18	25
181 — 210						1	0		2	1	4	0	6	7	10	6
211 — 240							0		2	3	0	1	5	5	8	6
241 — 270							0			1	0		3	3	0	1
271 — 300							1				2		4	2	1	1
301 — 330													1	0	0	0
331 — 360													1	1	1	1
361 — 390														0		
391 — 420														2		
Totals	271	274	274	273	274	273	274	273	274	274	274	274	274	273	274	272

The following facts are revealed by comparison of the results for the

TABLE II

MEANS, ETC., FOR EACH SEX, INTERVAL AND FILLING

		M.		M. V.		+V.		No. +V.		−V.		No.−V		R. V.	
		Males	Females	Males	Females	Males	Females	Males	Females	Males	Females	Males	Females	Males	Females
18″	I	17.7	20.74	5.4	10.35	5.8	15.48	15.9	22.75	5.7	7.78	20.0	45.00	30	49.83
	E	19.5	25.55	4.9	9.84	6.0	13.98	16.9	23.75	4.4	7.69	19.0	44.75	25	39.10
	R	15.5	18.46	4.9	9.14	6.3	13.03	14.6	24.25	4.4	7.07	21.2	44.25	31	49.26
	W	11.5	15.58	3.7	8.56	4.6	11.83	14.3	25.75	3.2	7.14	21.6	42.50	33	54.55
36″	I	33.3	42.81	9.1	16.55	12.6	20.63	14.2	28.00	8.1	14.01	21.7	40.50	27	38.32
	E	33.1	41.54	8.4	15.23	10.7	23.34	14.2	23.00	7.2	11.57	21.7	45.25	25	36.66
	R	32.1	41.71	8.4	16.42	9.7	20.78	16.3	27.75	7.5	13.84	19.6	40.75	26	38.84
	W	24.7	30.10	9.0	14.71	10.9	22.08	14.4	23.50	8.7	11.24	21.5	44.75	36	48.64
72″	I	63.3	73.00	17.2	27.20	20.9	33.72	15.7	27.75	15.5	22.83	20.2	40.75	27	36.87
	E	63.1	77.13	16.0	26.56	18.8	37.27	16.9	24.75	14.7	20.98	19.0	43.75	27	34.43
	R	57.9	70.78	17.3	30.30	20.9	39.28	15.2	26.75	15.7	24.91	20.7	41.75	30	42.77
	W	51.2	54.93	19.8	24.21	23.7	29.47	15.0	38.25	16.0	21.26	20.9	36.75	37	43.84
108″	I	92.7	113.37	29.8	40.13	35.3	44.08	15.2	32.00	25.9	37.87	20.7	36.50	32	35.34
	E	99.8	114.88	26.3	36.38	29.3	49.26	14.9	26.25	24.4	29.51	19.9	42.00	26	31.57
	R	90.1	100.47	28.3	40.18	33.9	47.90	15.3	34.25	24.8	34.60	20.6	39.75	31	39.95
	W	75.5	87.45	32.4	45.33	40.8	61.12	14.9	34.25	27.5	35.86	19.9	42.75	42	51.67

two sexes.[1] Without exception the means for the females are larger than those for the males. All but one of the sixteen intervals (E 18″) are underestimated by the males, whereas all but six are overestimated by the females. The amount of over- and under-estimation is given in Table 12. In every instance the females overestimate in comparison with the males. The mean variability is very much greater for the females, as is also the relative variability. If variability be taken as a measure of reliability of judgment the males are far superior to the females.

As is obvious from Table 12, both under- and over-estimation increase with increase in the length of the interval. For 18″ intervals they are least, for 108″ intervals greatest.

The influence of the fillings is marked. The writing intervals without exception are judged as shortest; reading gives the next shortest intervals, while sometimes idleness, sometimes estimating, comes third. In order of increasing length of average estimates of the intervals the fillings stand: writing, reading, idleness, estimating. As a rule the averages for the idleness and the estimating intervals are nearly the same, but it is worthy of note that the females always overestimate to a greater extent when estimating than when idle. This is another indication of the discrepancy between the female time-unit and the objective unit.

About ninety per cent of the subjects estimated by some counting method. The methods most frequently used were "counting seconds," counting "1 and 2 and 3 and 4, etc.," counting the swings of a pendulum, tapping, and counting imaginary watch-ticks.

The above statements might suggest that the overestimation characteristic of the female judgments is due to a more rapid counting rhythm. This, however, is not true, for the letter-counting tests indicate a slightly more rapid rhythm for the males, 93.42 as opposed to 91.89.

In Table 13 are the means, variabilities, errors, etc., for the letter-counting tests. In this table we have presented the values of the various statistical quantities for the several ages of both males and females, for there are certain interesting differences which should be noted. Similar age-tables have been prepared for all of the other results, but in no case have noteworthy differences appeared. From Table 13 it will be observed that the males on the average count more letters in

[1] The values of the means and variabilities was determined to the second decimal for the females, but only to the first for the males; consequently the relative variability could be computed exactly to the first decimal for the females and to the unit only for the males.

TABLE 12

AMOUNT OF OVER- AND UNDER-ESTIMATION OF INTERVALS

		Males.	Females.
18″	I	− 0.3	+ 2.74
	E	+ 1.5	+ 7.55
	R	− 2.5	+ 0.46
	W	− 6.5	− 2.42
36″	I	− 2.7	+ 6.81
	E	− 2.9	+ 5.54
	R	− 3.9	+ 5.71
	W	−11.3	− 5.90
72″	I	− 8.7	+ 1.00
	E	− 8.9	+ 5.13
	R	−18.1	− 1.22
	W	−24.8	−17.07
108″	I	−15.3	+ 5.37
	E	− 8.2	+ 6.88
	R	−17.9	− 7.53
	W	−32.5	−20.55

Males.

Age	17	18	19	20	21	22	23
Over- or under-estimation	− 3.60	− 7.50	−13.02	−13.49	− 8.71	−13.20	− 8.55
No. +	6	2	2	0	2	2	2
No. −	10	14	14	16	14	14	14

Females.

Age	17	18	19	20
Over- or under-estimation	+ 2.38	− 4.16	− 2.57	+ 2.55
No. +	11	8	8	12
No. −	5	8	8	4

+ = overestimation.
− = underestimation.

thirty seconds than do the females, and at the same time make more errors. The mean and relative variabilities for the sex-groups are almost the same. Curiously enough the number of letters counted as well as the accuracy of counting decrease for the females with age (17 to 20 years being the age-limits of the group under consideration). The males within the same age-limits increase in rapidity of counting, but decrease in accuracy. In the examination of Table 13 it is to be remembered that the 17 and 23 year groups of males contain only 16 individuals each, and therefore cannot be compared to advantage with the other groups. Both mean and relative variabilities decrease from 17 to 20 years for the females, whereas for the males there is no constancy in the direction of the change.

TABLE 13

MEANS, ETC., FOR LETTER-COUNTING

Males.

Age	17	18	19	20	21	22	23	*Average*
M.	92.81	92.48	94.00	95.87	92.66	97.06	89.06	93.42
M. V.	10.29	15.31	16.50	12.90	7.81	13.04	19.20	13.58
+V.	11.76	22.96	16.50	11.70	8.13	16.30	21.94	15.61
No. + V.	7	9	20	37	24	14	7	16.96
−V.	9.15	11.48	16.50	14.40	7.51	10.87	17.09	12.57
No. −V.	9	18	20	30	26	21	9	19.00
R. V.	11.1	16.5	17.6	13.5	8.4	13.4	21.6	14.59
Errors	0.88	1.04	2.10	3.03	1.70	0.89	2.75	1.77

Females.

Age	17	18	19	20	*Average*
M.	97.74	95.51	89.21	85.09	91.89
M. V.	17.06	14.56	11.30	11.28	13.55
+V	18.67	18.49	12.06	14.56	15.94
No. + V.	32	21	30	31	28.50
−V.	15.71	12.14	11.66	9.21	12.18
No. −V.	38	32	31	49	37.50
R. V.	17.5	15.2	12.7	13.3	14.70
Errors	1.35	1.46	1.49	1.65	1.47

SUMMARY

(1) The length of a second is slightly overestimated by men, greatly underestimated by women.

(2) Intervals of from 18 to 108 seconds are usually slightly underestimated by men (ages 17 to 23 years), and greatly overestimated by women (ages 17 to 20 years).

(3) The time-estimates of women are far more variable than those of men, and on the whole markedly less accurate.

(4) Both men and women favor estimates which end in 0 or 5, as well as simple fractions of a minute, but the tendency is stronger in women than in men. Over one third of the estimates reported in this paper were 15 seconds or simple multiples thereof.

(5) In letter-counting the groups of subjects studied (251 men and 274 women) exhibited differences just the opposite of those in time-estimation, for the men counted more rapidly and less accurately than did the women.

(6) Of the four fillings for the intervals used in the experiments, "writing" gave the smallest estimates of the intervals, listening to "reading" next, while "idleness" and "estimating" were conditions in which the intervals seemed much longer to both men and women.

(7) This preliminary study of sex-differences in time-estimation, by which it has been learned that women overestimate and are notably inaccurate in comparison with men, is to be followed and supplemented by the results of an investigation now in progress concerning the relations of sex-differences in time-estimation to age and physiological rhythms.

ASSOCIATIONS UNDER THE INFLUENCE OF DIFFERENT IDEAS

BY BIRD T. BALDWIN

The purpose of the following investigation was to study the influence of two or more starting-points on the train of associated ideas. It was begun in October, 1902, and concluded in January, 1905. Nineteen graduate students acted as subjects, and the experiments were conducted with each individually for one hour per week, except in a few instances where two subjects were present together. Occasionally the experimenter acted as subject in order to get a clearer insight into introspective data. There are recorded here thirteen groups of one hundred and eight sections, including eight hundred and fifty-five separate experiments, with a sum total of eleven thousand, one hundred and thirty-five named associations.

So far the field has not been studied experimentally, although Cordes,[1] while attempting to observe the effect of an unconscious intermediating factor in 'mediate association,' noticed that the accompanying factor (particularly with a tone) sometimes combined with the starting-point in determining the associated series. The fact is simply mentioned, and Scripture's [2] conclusions from the same are inadequate when he states, "In general we may say that two simultaneous ideas have an effect that depends on their relative masses; if one of the ideas is overpoweringly weighty the next idea will be chiefly influenced by it, but if the two are nearly balanced the next idea will be the result of the two." Miss Calkins [3] makes a near approach to the problem in a study of "Mental Combination," where auditory words were given as nearly simultaneously as possible and the subject was required to remain for four seconds in silence and then to write the train of imagery which passed in six following seconds; the words as auditory starting-points were finally excluded from the experiments "because the first word

[1] Cordes, G.: Experimentelle Untersuchung über Association, Phil. Studien, vol. 17, p. 30.

[2] Scripture, E. W.: Elements of Experimental Phonetics, p. 142.

[3] Calkins, M. W.: Memory and Association, Psychol. Rev., vol. 5, p. 451.

pronounced tends often to establish itself so firmly that its association-images are proof against the intrusion of the second word, which has therefore no chance to be grasped with the first." These results differ from the ones here recorded which were obtained independently and under different conditions. In her valuable monograph on Association[1] there is in one instance a suggestion of the problem, but no results are given.

The two or more starting-points were nonsense syllables, concrete or abstract words, pronounced by the investigator or shown on cards, or finally, pictures. The associations continued for a period of fifty seconds unless otherwise indicated. The same letters denote the same subjects throughout all the experiments, K. and R. being women. In the examples given, Roman numbers have been used, in place of the letters, as it was found impossible to get representative series which were entirely devoid of personal references.

After the associations had been "jotted down," the subjects in each case copied them, noting also the suggesting ideas, and giving, in many cases, a number of introspective notes. It is important to mention that in all of the work these records of suggesting ideas were made by the subjects without any questions on the part of the investigator.

Group I. Two Nonsense Syllables

In order that a standard might be obtained that would serve as a basis for subsequent experiments, two nonsense syllables were pronounced with equal emphasis, as starting-points, and the subject was asked merely to wait until both had entered consciousness and not to favor nor inhibit either. If the subject stated that associations arose between the pronunciation of the starting-points the results were discarded.

Turning to the notes we find that K. reported, "The first just hovered around," and the associations followed the second, or as V. states it, "Sof was carried subconsciously for a while without influence," while By. frequently noticed that "The first may be in the background on the point of breaking in but be inhibited involuntarily," or in another, "It was frequently present but exerting no influence." A. "forgot what the other word was, but felt it trying to get a hand in." Ro. goes further and states in terms similar to A., "The words which might have been suggested by 'taf' were wholly inhibited, though I am sure 'taf' was present to me throughout the series, but was not efficacious as

[1] Calkins, M. W.: Association, Psy. Monograph, no. 2, p. 46.

against the other syllable." It is unnecessary to multiply instances to show that the wraith, so to speak, of one may linger for a short time, or occasionally for the entire series, but that the associations are determined by the other. Or the lingering wraith of a starting-point may hover ineffectively in consciousness and exert no determining influence on the series until toward its close; then, however, although both starting-points still persist, they exchange places so far as effectiveness is concerned, and the former wraith becomes the primary factor in effecting the series of associations. For example, "The two syllables were present all the way through, but the former exhausted itself as a word-suggester after a few words." Again Ro. gives examples where "One alone may be in consciousness and determine the associations when the other may enter without effect upon the series, or may determine the series, the first persisting but losing its effect"; and a case of both ideas persisting and each alternately exerting an influence was noted by M. These last two conditions are exceptional.

Studying carefully the notes and graphic representations as given in the tables, it is apparent that one starting-point may be followed independently and exclusively; we find "'fef' suggesting 'theft,' and the subsequent associations monopolizing the field of consciousness and 'tuz' not again appearing." (V.) The results show that in the one hundred and seven experiments one was followed exclusively only twenty times. The following would be an example:

III. Naf-Tam

— tambour	tam
— experiment	tambour
— psychology	experiment
— laboratory	experiment
— space	psychology
— time	space
— ego	space and time
— Ebbinghaus	ego
— discussion	psychology
— posted notice	psychology
— door	posted notice
— gray	door
— stairs	door
— fishes	laboratory
— water	fishes
— decay	fishes
— animal	fishes
— meat	fishes
— odor	meat

In other cases one starting-point may be followed and then the other for a few or many associations, each "occupying the mind to the exclusion of the other for a number of words, when suddenly the other appears and suggests a new series," which V. calls "a curious zigzagging in consciousness." This may take place at any point along the series. As Ro. indicates, "There was rivalry here; 'yud' wholly inhibited all the 'zid' associations, even 'zid' itself for the time, then, 'yud' apparently being exhausted, 'zid' entered again into the focus."

In contrast to the above forms, the tables indicate and the notes verify that while the associations primarily follow one there can be traced the modifying effect of the other; "The nature of the words was influenced by a fringe of consciousness which contains the other starting-point," M.'s comment runs, while J. adds, "The other helped in the mental picture but had no special significance." There are no experiments in the first group where this partial fusion took place throughout the series, and there are only six examples where it took place before the sixth word and continued to the end. While there are no cases here of alternating partial fusion, yet it sometimes happens, that is, the predominating influence is sometimes transferred from one to the other. The modifying influence may reach such a degree at any point in the series that the identity of each point of departure of the association is lost and there is total fusion, resulting in one idea or one train of thought. For example, 'rel' and 'mem' gave 'realm' for V.; for Ht. 'fef' and 'tuz' gave 'fezz'; for M. they gave 'fez'; for Ro. 'fuz.'

Of the twenty-one experiments where one starting-point was followed exclusively, there were two cases for the first and nineteen for the second. There were but two cases of alternating groups, a third one being unique in that after the first three words there was a persistent alternation between two apparently independent series of inter-related words and inter-related visual images. There were three cases of complete fusion throughout the series and nine where total fusion started before the fifth word and continued.

In a number of cases the initial starting-points were forgotten before the end of the series and the subjects were sometimes unable to recall them.

As the subjects were kept ignorant of the characteristic persistence of either starting-point in forming separate associations, and of the tendency towards fusion, as well as of the preponderance of influence of one over the other, it is to the tabulated results that we must look

for quantitative measurement of these. 76.2 % of the associations are due to the independent influence of one or the other of the starting-points; 23.9 % are due to the combined influence; for the first starting-point we have 18.7 %, for the second 57.5 %. In 15.3 % of all the associations the combined influence was the result of total fusion; in 4.1 %, the result of a union where the first starting-point predominated in influence; in 4.4 %, the second predominated. The following graphic representation indicates the order of influences for the first pair of syllables. The table gives the results for the ten pairs.

TABLE I. TWO NONSENSE SYLLABLES SPOKEN

Time — 50 seconds.

Characters: | represents the influence of the first, — the second, + complete fusion, α fusion with the first predominating, β fusion with the second predominating.

(1) Taf — Coz

M. | — — — — | | | | | — — — —

F. — — | | + + + + | | | + + β β β β β β

H. — | | | | | | | | | | | | | | |

R. | — — — — — — — — — — — — — — —

K. — — — — — — —

V. | | — — — — | — — | + + + +

Ro. | | | — — — — — — — — — — — — — — — —

Bl. — — — — — — — — — — — — — — —

By | — | | | — — — — — — — — — — — — — — — — — — —

Bs. — — — — — — — — — — — — — — —

Ht. — — | | | | | | | | | | | | |

Starting-points:

(1) Taf–Coz.	(6) Fef–Tuz.
(2) Cim–Bef.	(7) Sar–Nef.
(3) Yud–Zid.	(8) Sek–Lub.
(4) Sof–Deb.	(9) Hov–Bes.
(5) Naf–Tam.	(10) Rel–Mem.

	I	—	+	α	β
M.	20 16.5 %	69 56.6 %	26 21.2 %	4 3.3 %	3 2.4 %
F.	28 22.2 %	22 17.5 %	37 29.4 %	0	39 30.9 %
H.	39 26.3 %	98 66.2 %	0	11 7.5 %	0
R.	9 6.3 %	102 71.4 %	32 22.3 %	0	0

TABLE I — *continued*

K.	47	58	1	14	0
	39.1 %	48.3 %	.7 %	11.9 %	
V.	30	79	54	0	0
	18.4 %	48.4 %	33.2 %		
Ro.	32	111	0	5	0
	21.4 %	75.1 %		3.5 %	
Bl.	5	131	15	15	0
	3.1 %	78.9 %	9.0 %	9.0 %	
By.	43	102	23	15	22
	16.8 %	52.6 %	11.7 %	7.7 %	11.2 %
Bs.	9	72	31	0	6
	11.4 %	58.5 %	25.3 %		4.8 %
Ht.	32	58	20[1]	0	0
	29.0 %	52.8 %	18.2 %		
Totals	294	902	239	64	70
	18.7 %	57.5 %	15.3 %	4.1 %	4.4 %

Number of subjects, 11; number of sections, 10; number of experiments, 110; number of associations, 1569.

GROUP II

Two concrete nouns with apparently equivalent connotation were pronounced. In order that the subject get no clue as to the preponderance of one starting-point over the other, the nine sections were given at irregular intervals in connection with other experiments.

The tables show few cases of fusion, there being but one case in the eighty experiments where all the associations were the result of the combined influence of both starting-points. There was no other instance where complete fusion took place before the sixth association and continued throughout, and only six cases where any form of fusion took place in the eighty experiments within the first five words (7.6 %), while in the one hundred and seven experiments of Group I there were twenty-five (23.9 %).

The tables show a tendency which was noted when two syllables were used and which is emphatically brought out here, namely, it is the position or sequence which determines which of the two equivalent starting-points shall produce the greater influence. When the two starting-points are given in immediate succession, it is the second which predominates in influence. This preponderance may be clearly demonstrated by the tables alone, though there are many notes which

show "the greater influence of one" (the second). A. seems to have realized this when he wrote, "While the first was uppermost the second hovered in subconsciousness, resting content, knowing that it would have its turn soon." In corroboration the tables give — abstracting from all cases of fusion — 268 words (23.2 %) for the first, 721 words (62.3 %) for the second. In fifty-five of the eighty experiments the last had full control at the end of the series. Taking the cases where one was followed exclusively throughout the series, starting between the first and the sixth named association, we find eight for the first and twenty-nine for the second. This greater influence is again to be found in the cases of partial fusion; the first predominating in 2.6 %, and the second in 4.3 %.

The results of both groups show that the starting-points tend in a high degree toward independent influence, and also that such a method of presentation is one of sequence rather than simultaneity, as the two words show unequal influence. The former must in a manner be reproduced to become a point of departure for associations, while the latter acts directly. Is it then a condition of mind that when similar impressions are presented the one in the presence of which consciousness is reacting directly has a greater influence in arousing associations than one which is just past? This we are forced to conclude is the case, but the proof of the conclusion will be supplemented by later results.

TABLE II. TWO WORDS SPOKEN

Time — 50 seconds.
Characters — same as Table I.

(1) Library—River

```
M.   | + +  β— — — — — — — — — — — —
F.   | — — — — — — — — — | | | |
H.   — — — — — — — — — — — — — — — —
Ro.  | | | | — — — — — — — — —
J.   | — | | | | — — — — — — — — — — — — — — —
Bl.  | | — — — — | | | | | | | | | — — /
By.  — — — — — — — — — — — — — — — — — — — — —
Bos. | — — — — — — —
Bur.— + — — — — — — — — — — —
```

(1) Library — River.
(2) Hat — Road.
(3) Newspaper — Medicine.
(4) Bicycle — Drum.
(5) Theatre — Magazine.
(6) Store — Church.
(7) Soldier — House.
(8) Ship — Boy.
(9) Furniture — Tree.

TABLE II — *continued*

	|	−	+	α	β
M.	32	66	26	0	13
	23.4 %	48.2 %	18.9 %		9.5 %
F.	32	67	13	5	1
	27.2 %	56.7 %	11.0 %	4.3 %	.8 %
H.	59	79	5	0	0
	41.3 %	55.2 %	3.5 %		
Ro.	44	85	0	0	0
	34.1 %	65.9 %			
J.	21	121	8	5	0
	13.5 %	78. %	5.3 %	3.2 %	
Bl.	22	83	6	0	1
	19.7 %	74.1 %	5.4 %	.8 %	
By.	16	97	18	14	23
	9.5 %	57.8 %	10.7 %	8.3 %	13.7 %
Bs.	26	64	3	7	1
	25.8 %	63.5 %	2.9 %	6.9 %	.9 %
Bur.	16	59	8	0	10
	17.2 %	63.4 %	8.6 %	0	10.8 %
Total	268	721	87	31	49
	23.2 %	62.3 %	7.6 %	2.6 %	4.3 %

Number of subjects, 9; number of sections, 19; number of experiments, 80; number of associations, 1156.

GROUP III

For two syllables and like words the second strongly predominates in awakening associations; will the same be true when two simple outline pictures are shown in the same order? The following results show in attestation of the above conclusion percentages remarkably similar to those of the words. There are 23.7 % for the first and 61 % for the second; 84.7 % of the associations show no sign of fusion and only 15.3 % for fusion and the different forms of partial fusion, the second still holding the ascendency. When the two pictures did fuse, R. tells us, "All the associations were more elaborate pictures than when mere words were given."

A comparative study, inadequate though it is, offers a partial parallelism between the predominating memory type and stimulation. The subjects who are preëminently of the visual, K., visual motor, M., H., V., and visual lingual motor, Ht., find the pictures more suggestive

than the syllables, while those of the pure motor, F. and By., reverse the order and find that the syllables offer more numerous and "more vivid" (F.) associations.

Common experience, however, immediately shows the limitations of reaching conclusions, when considering prolificacy as criteria of suggestiveness, inasmuch as starting-points which are abundantly rich in associations tend to produce so many points of departure that they tend to inhibit one another. Again there is to be considered the kind of associations, those of the syllables being of a very elementary character and in serial form.

A few experiments were given in which colored slips of paper were used as starting-points. These proved very suggestive for the subjects. Also a few tones were given, but these were soon discontinued, as other tones, which could not be recorded, were frequently suggested. The sentence which forms a very satisfactory starting-point where one is used could not be used to an advantage where several were given. As we are here interested in the mutual influence of the starting-points, our remaining study will be confined to a quantitative and qualitative variation of the forms used in the previous groups.

Table III. Two Pictures Shown

Time — 50 seconds.
Characters — same as Table I.

(1) Boy Rolling Hoop — Blacksmith

```
M.  | | | – – – – – – | | | | |
F.  | | – – | + +
H.  – – – – – – – – – – – –
R.  – – – – – – – – – – – – – – – –
K.  – – – – – – – – – – – – – – – – –
V.  – – | | + + + + + + + + + + + + + +
S.  – – – – – – – – – | | | | | | |
By. | – – – – – – – – – – – – – – – – – – – – –
Bs. – – – – – – – – –
Ht. | | – – – – – – – –
```

(1) Boy Rolling Hoop — Blacksmith.
(2) Old Man With Umbrella — Bird House.
(3) Carpenter — Mower.
(4) Children Playing — Boy with Basket.
(5) Horse — Dog House.
(6) Shoemaker — Fisherman.
(7) Little Girl — A Chicken.
(8) Boy — A Sheep.

	|	–	+	α	β
M.	41	65	2	0	1
	37.6 %	59.7 %	1.8 %		.9 %

TABLE III — *continued*

F.	11 13.9 %	42 53.2 %	21 26.6 %	0	5 6.3 %
H.	37 27.4 %	52 38.5 %	11 8.1 %	26 19.3 %	9 6.7 %
R.	41 33.4 %	58 48 %	8 6.5 %	0	15 12.1 %
K.	32 27.1 %	82 72.9 %	0	0	0
V.	12 8.8 %	95 70 %	21 15.4 %	3 2.1 %	5 3.7 %
S.	36 45. %	44 55. %	0	0	0
By.	8 5.4 %	111 77.2 %	12 8.3 %	0	13 9.1 %
Bs.	13 21.7 %	40 66.7 %	7 11.6 %	0	0
Ht.	22 20.9 %	72 68.6 %	7 6.7	0	4 3.8 %
Totals	253 23.7 %	661 61 %	89 8.2 %	29 2.7 %	52 4.4 %

Number of subjects, 10; number of sections, 8; number of experiments, 76; number of associations, 1084.

GROUP IV

The previous experiments suggest the conclusion that that starting-point in the presence of which consciousness is reacting exerts a greater influence than one just past. The proof of this anticipated result is supplemented by subsequent consectary data.

It is necessary to test the outcome when the nature of the impressions is varied and the starting-points are given simultaneously. An outline picture and a word may be so given. These experiments verify from a different standpoint the statement that the picture establishes itself more permanently and is more influential, there being 47.8 % of the associations produced by the picture alone, 14.8 % by the words alone, 25 % of a fusion of the two, and 12.4 % of a uniting influence with the first predominating. The subject R. is an exception in that she favors the word when not favoring a combina-

tion of the two, a fact which I am unable to explain except to add that the subject stated that the æsthetic pleasure connected with the picture was sufficient to inhibit the associations. K. has 100 % for the pictures, which is partially explained by the fact that she is a remarkable visualizer who reproduces all situations in visual terms.

TABLE IV. PICTURE SHOWN AND WORD SPOKEN

Time — 50 seconds.
Characters same as Table I.

(1) (a rabbit) — Table

M. | − − | | | | | | | + − − − −
R. + + + + + + + + + + + + + + + + + +
K. | | | | | | | | | | | | | | |
S. | | | | | − | | | | |
J. | − − | | | | | | | | | − − − | | | | | | | | |
Ht. | − | | | | | | | | | | | | − − − −

(1) (a rabbit) — Table.
(2) (horse) — Book.
(3) (boy) — Dish.
(4) (duck) — Iron.
(5) (dog) — Paper.

	|	−	+	α	β
M.	38 55.0 %	9 13.1 %	16 23.3 %	6 8.6 %	0
R.	1 1.3 %	20 26.3 %	55 72.4 %	0	0
K.	65 100.0 %	0	0	0	0
S.	29 50.8 %	15 26.3 %	3 5.3 %	10 17.6 %	0
J.	75 64.2 %	19 16.3 %	14 11.9 %	9 7.6 %	0
Ht.	17 19.5 %	7 8.1 %	30 34.5 %	33 37.9 %	0
Totals	225 47.8 %	70 14.8 %	118 25.0 %	58 12.4 %	0

Number of subjects, 6; number of sections, 5; number of experiments, 30; number of associations, 471.

Group V

The character of the starting-points determines which shall be followed in a simultaneous presentation; can the character of two immediately successive starting-points be so varied as to overcome the factor of temporal difference? The few experiments in this group show an attempt to shift and measure this influence, selecting as variants words of general and specific character, the first being a term of everyday experience with the subjects, of wide connotation, and therefore presumably rich in associations, and the second, which had the advantage of position, one of more limited and specific connotation, thus in a measure making it possible to test the stability of the second in establishing itself as a word-suggester. For the words "Gymnasium — Stamens" the second still strongly predominates. There is a slight increase in the amount of fusion (24.5 % in all), the explanation of which would probably lie in the fact that the first is more assertive than before and offers more elements common to the two. There was one case of total fusion, one case where fusion started before the sixth word and continued, no cases of alternating fusion, four cases of partial fusion, five cases where the second was followed throughout, no case where the first was followed throughout, five cases of alternation between the first and the second. In 75.5 %, the influence was exerted without fusion. Eight of the nine subjects favor the specific term.

Table V. General and Familiar Word — Specific Word Spoken

Time — 50 seconds.
Characters — same as Table I.

(1) Laboratory — Rake

M.	| | | | | — — — — — — — — — — — — — — — —
F.	| — — — — — — — — —
H.	+ + + + + + + + + + +
V.	| — — — — — — — — — — — — — | | | | |
S.	— — — — — — — — — — | | | |
Bl.	— — — — — — — — — — — — — — — — —
By.	| — — | | | | | | | | |
Bs.	
Ht.	— — — — — — — —
J.	| | β β β β β β β β β β β β | | | | |

(1) Laboratory — Rake.
(2) Experiment — Butterfly.
(3) Gymnasium — Stamens.

TABLE V — *continued*

	|	–	+	α	β
M.	22 38.6 %	25 43.9 %	4 7.0 %	6 10.5 %	0
F.	4 17.4 %	19 82.6 %	0	0	0
H.	4 9.5 %	25 59.6 %	13 30.9 %	0	0
V.	7 12.8 %	38 70.7 %	2 3.7 %	0	7 12.8 %
S.	11 24.4 %	34 75.6 %	0	0	0
Bl.	0	36 100.0 %	0	0	0
By.	11 25.5 %	16 37.1 %	7 16.5 %	0	9 20.9 %
Bs.	0	9 39.1 %	12 52.2 %	2 8.7 %	
Ht.	10 32.3 %	11 35.4 %	10 32.3 %		
J.	17 32.1 %	8 15.1 %	15 28.3 %	0	13 24.5 %
Totals	86 21.2 %	221 54.3 %	63 15.4 %	8 1.9 %	29 7.2 %

Number of subjects, 10; number of sections, 3; number of experiments, 27; number of associations, 407.

GROUP VI

What modification of influence takes place when an auditory impression in the form of a word of abstract nature (the pure abstract words are taken later) is given in comparison with a concrete noun? The tables indicate that we are now able partly to overcome the disadvantage of first position by the advantage of concrete content. 24 % of the associations are the result of total fusion; there are eight cases of total fusion throughout the series, and seven where total fusion took place before the third word and continued. There are two cases where the first was followed throughout, and but one where the second was followed exclusively. The tables indicate that there were very few words. Some of the subjects claim, "These words did not seem equally rich in associations. I was not at all conscious of

the one while the other was in consciousness." (H.) The most characteristic feature here was, as Br. also indicates, "Great amount of rivalry at the beginning of the series"; while Bs. states, "There was a lot of confusion and a feeling of groping for words." Br. adds later, "For some seconds association seemed obstructed. Then by an effort the process was started which followed an involuntary course. A kind of confused presence of both words." Another subject adds, "There was a long blank after the words were said in which both words were balancing off in the fringe of consciousness and the mind expectant, passively waiting for an association to turn up. The hesitant period seemed marked by an attempt at a synthesis of these two words in some way."

Subject XIX

Lamp — Justice.

| chimney ....lamp.
| white ....lamp chimney.
| yellow ....white chimney.
| flame ....yellow.
| nickel ....lamp plus the other words.
— scales ....justice.
— purple robe ....justice.

As the tables indicate, there was frequently a strong tendency here for the abstract terms to fuse. As H. noticed, "It does not tend to call up associations of its own stripe, but in some way becomes concrete."

Table VI. Two Words: Concrete — Abstract Spoken

Time — 15 seconds.
Characters — same as Table I.

(1) Desert — Hate

A.	|	|	|	+	+	+			
M.	|	|	|	|	—	—			
F.	+	+	+						
H.	|	—	|	|	|	|	|	|	|
Ro.	|	|	—	—	|	|	|	|	
Bl.	+	+	+	+	+	+	+		
By.	|	|	|	+	+	+	+	+	+
Bs.	—	|	|	|	|	|			
Br.	|	|	|	+	+				

(1) Desert — Hate. (3) Pen — Fatigue.
(2) Lamp — Justice. (4) Gate — Fear.

TABLE VI — *continued*

	|	−	+	α	β
A.	10 55.5 %	2 11.2 %	6 33.3 %	0	0
M.	7 28.0 %	8 32.0 %	10 40.0 %	0	0
F.	3 18.7 %	8 50.1 %	5 31.2 %	0	0
H.	17 50.0 %	16 47.1 %	1 2.9 %	0	0
Ro.	15 57.7 %	11 42.3 %	0	0	0
Bl.	4 14.2 %	15 53.5 %	7 25.1 %	2 7.2 %	0
By.	4 11.8 %	21 61.8 %	8 23.5 %	0	1 2.9 %
Bs.	5 21.8 %	1 4.3 %	15 65.2 %	0	2 8.7 %
Br.	11 52.3 %	8 38.1 %	2 9.6 %	0	0
Totals	76 33.8 %	90 40.0 %	54 24.0 %	2 .8 %	3 1.4 %

Number of subjects, 9; number of sections, 4; number of experiments, 34; number of associations, 225.

Reversing the order by placing the concrete noun second, it gains in influence. We are told by the subjects at this point, "The choice seems to be determined by the concreteness of the word." (H.) "The abstract soon exhausted itself as a word-suggester." (Ro.) There was fusion in 24 % of the associations of the first group, and 22.2 % in the second. There are six cases where the second alone prevails, two for the first starting-point and four for the second. There are eleven cases of fusion beginning before the sixth word and continuing throughout the series. There is much partial fusion, with the second predominating in influence.

The results again emphasize the fact that the influence is transferable, also that normally the second has the advantage; furthermore, they illustrate the preponderance of the concrete word as a starter of associations, and that the abstract term when it exerts an influence tends to fuse rather than persist in having separate associations; all of which shows that concrete terms produce more vivid impressions than abstract ones, and would, when it is possible to use them, be of direct aid to the learner.

TABLE VII. TWO WORDS: ABSTRACT — CONCRETE SPOKEN

Time — 15 seconds.
Characters — same as Table I.

(1) Honesty — Tide

A.	– | – – – –
M.	– – – – | | | |
F.	| – – – – –
H.	| – – –
Ro.	| | – – – –
Bl.	| – – – – – – –
By.	– – + +
Bs.	– – – – – – –
J.	– + + + + + +

(1) Honesty — Tide.
(2) Skill — Coal.
(3) Terror — Sky.
(4) Refined — Flag.

	|	–	+	α	β
A.	2	7	11	0	4
	8.4 %	29.2 %	45.8 %		16.6 %
M.	4	11	10	1	5
	12.9 %	35.5 %	32.3 %	3.2 %	16.1 %
F.	2	8	9	0	0
	10.5 %	42.2 %	47.3 %		
H.	8	11	1	3	0
	39.2 %	43.5 %	4.1 %	13.2 %	
Ro.	10	13	3	0	0
	38.4 %	50.0 %	11.6 %		
Bl.	15	10	0	0	0
	60.0 %	40.0 %			
By.	2	20	3	0	0
	8.0 %	80.0 %	12.0 %		
Bs.	1	11	6	0	2
	5.0 %	55.0 %	30.0 %		10.0 %
J.	6	16	6	0	0
	21.4 %	57.2 %	21.4 %		
Totals	50	107	49	4	11
	22.6 %	48.4 %	22.2 %	1.9 %	4.9 %

Number of subjects, 9; number of sections, 4; number of experiments, 36; number of associations, 221.

Increasing the disparateness by making the one a proper name and the other a pure abstract noun, we find the name dominates consciousness, almost to the exclusion of the abstract term. The tables confirm the conclusions that the abstract term, even when given the advantage of position, exerts little influence, for in the first group of eighteen experiments of two hundred and eighty-four associations there are 13.4 times as many associations for the first as for the second, or two hundred and fifteen words for the first (75.8 %) and sixteen (5.5 %) for the second.

Reversing the order, the burden of influence swings back to thirty-eight (12.8 %) for the first and one hundred and fifty-six (52.7 %) for the second with an amount of fusion increased to ninety-nine (33.4 %).

Table VIII. Two Words: Proper Noun — Abstract Noun Spoken

Time — 50 seconds.
Characters — same as Table I.

(1) Lowell — Liberty

M. | | | | a a a | | | | | |
R. | a a | | | | | | | | | | | | |
K. | | | | | | | | | | | | | |
J. | | | + + + a a a a a a a a a a a a a
S. | | | | | — | | | | | | |
Ht. — — | | | | | | | | | | | | | |

(1) Lowell — Liberty.
(2) Roosevelt — Fidelity.
(3) Eliot — Integrity.

	|	—	+	a	β
M.	29 70.7 %	3 7.4 %	2 4.8 %	7 17.1 %	0
R.	30 61.2 %	0	17 34.7 %	2 4.1 %	0
K.	43 95.5 %	2 4.5 %	0	0	0
J.	42 66.6 %	0	8 12.7 %	13 20.7 %	0
S.	40 95.2 %	2 4.8 %	0	0	0

TABLE VIII — *continued*

Ht.	31 70.4 %	9 20.4 %	3 6.9 %	1 2.3 %	0
Totals	215 75.8 %	16 5.5 %	30 10.7 %	23 8.0 %	0

Number of subjects, 6; number of sections, 3; number of experiments, 18; number of associations, 284.

TABLE IX. TWO WORDS: ABSTRACT NOUN — PROPER NOUN SPOKEN

Time — 50 seconds.
Characters — same as Table I.

(1) Individuality — Lincoln

```
M.  | + | + + + + + + + + + + + +
R.  - - - - - - - - - - - - - - - -
K.  - - - - - - - - - - - -
J.  | - - - - - - - - - - - | | | | | | | | | |
S.  - - - - - - - - - - - - - - -
Ht. - - - - - - - - - - - - - - - - - -
```

(1) Individuality — Lincoln.
(2) Brevity — Webster.
(3) Justice — Hanus.

	|	—	+	a	β
M.	4 8.3 %	19 39.6 %	25 52.1 %	0	0
R.	5 10.4 %	31 64.5 %	12 25.1 %	0	0
K.	14 31.2 %	31 68.8 %	0	0	0
J.	13 19.1 %	11 16.1 %	44 64.8 %	0	0
S.	2 4.8 %	40 95.2 %	0	0	0
Ht.	0	24 53.4 %	18 40.0 %	0	3 6.6 %
Totals	38 12.8 %	156 52.7 %	99 33.4 %	0	3 1.1 %

Number of subjects, 6; number of sections, 3; number of experiments, 18; number of associations, 296.

Group VII

Occasional experiments of the previous groups indicated an abnormal influence of those ideas which were accentuated by the special interests of the subjects. We ask therefore: What is the relation of the newly aroused associations to the present content of consciousness? May the present content of consciousness be so varied as to reënforce or inhibit the characteristic influence of the individual impressions successively presented? In order to test this, words or a sentence were pronounced just previous to the presentation of the two starting-points related to them as follows; in the first divisions the preparatory words lead to the second, in the second divisions to the first, in the third divisions to neither, in the fourth divisions to both, and in the last we have a slight change in the marginal setting and a sentence leads to the second.

The following are examples of the first and second divisions:

Quartz — Granite — Shale.	Dog — Sheep — Horse.
Horn — Slate.	Squirrel — Telegram.

Subject XVIII		Subject XII	
— slate	slate	hunt	squirrel (and dog)
— slate ledge (Col.)	slate	pasture	sheep, horse, and hunt
— Great Falls, Mont.	quartz, granite, shale	yard	squirrel and pasture
— geology	quartz, granite, shale, G. F.	Harvard	yard
— Will B.	geology	Freshman	Harvard
— State University	Will B.	themes	Freshman and squirrel
— Sexton James	State University	hunting	themes, dog, "
— Mrs. J.	Sexton James	George	hunting
— C. S. J.	Mrs. J.	squirrel	George
— Indian School	C. S. J.	creatures	squirrel
— Fort Shaw School	Indian School	animals	creatures
— Mrs. C.	Ft. Shaw School	activity	" animals
— Mrs. E.	Mrs. C.	agility	" "
— Seattle, Wash.	Mrs. F.	grace	" "
— Miss M.	Seattle, Wash.	cuteness	" "
— Everyman	Miss M.	pets	" "
— Cousin V.	Everyman	civilisation	creatures
— theatre	Cousin V.	vs. cats	"
		enemies	cats

That one starting-point establishes itself more firmly, and offers more dominant associations with an increased degree of suggestiveness

is intimated by various expressions of the subjects who state, "the preparatory words called forth associations connected with this or that starting-point"; or "there was a felt fusion of all"; "a summation of all"; or "the preparatory words had an influence throughout"; "they strengthened this or that starting-point"; "they affected one and not the other"; and many similar expressions.

The influence of these preparatory marginal settings was also indicated very often by the nature or kind of words in the series.

Turning to the tables which collectively represent in a graphic and quantitative form the notes of the subject, and which are in harmony with the above, we find they demonstrate that there is a very intimate and definite relationship. When the state of mind immediately preceding the moment of the formation of the associated series is conditioned by the preparatory words leading to the second, the amount to which that starting-point dominates consciousness in arousing associations is greater than in any previous case where the words of like nature are pronounced.

With the preparatory words leading to the second starting-point, we have 11.5 % of the influence for the first, 69.2 % for the second; with them leading to the first, 58.2 % for first, 17.2 % for second; with them leading to neither, 40.6 % for first, 44.2 % for second; with them leading to both, 16.1 % for first, 54.8 % for second; with a preparatory sentence leading to second, 12.9 % for first, 59.7 % for second.

A few experiments were made where one word was given as a setting and four or eight starting-points were shown, but the starting-points were so numerous that they tended to confuse the subject's introspective account.

Table X. Two Words Shown: Three Preparatory Words Spoken Leading to the Second

Time — 50 seconds.
Characters — same as Table I.

Corn, Wheat, Oats. (1) Mob — Hay

M.	+	+	+	−	+	+	−	−	−	−						
R.	−	−	−	|	+	+	a	+	β	−	−	−	−	−		
K.	−	−	−	−	−	−	−	−	−	−	−	−	−	−		
S.	+	+	+	+	−	−	−	−	−	−	−	−	−	−	−	−
J.	+	+	+	+	+	+	β	β	−	−	−	−	−	−	−	
Ht.	−	+	+	+	+	β	β	β	β	β	β	β	β			

Table X — *continued*

(1) Corn, Wheat, Oats. Mob — Hay.
(2) Cloud, Mist, Dew. Oration — Canoe.
(3) Turtle, Fish, Frog. Key — Net.
(4) Quartz, Granite, Shale. Horn — Slate.
(5) Geometry, Plane, Rectangle. Case — Cube.

	|	—	+	α	β
M.	10 13.3 %	41 54.7 %	17 22.7 %	0	7 9.3 %
R.	15 19.3 %	37 47.4 %	21 26.9 %	1 1.2 %	4 5.2 %
K.	0	55 100.0 %	0	0	0
S.	8 10.5 %	64 84.2 %	4 5.3 %	0	0
J.	17 18.1 %	69 73.4 %	6 6.3 %	0	2 2.2 %
Ht.	2 2.8 %	45 62.5 %	17 23.6 %	0	8 11.1 %
Totals	52 11.5 %	311 69.2 %	65 14.4 %	1 .2 %	21 4.7 %

Number of subjects, 6; number of sections, 5; number of experiments, 29; number of associations, 449.

Table XI. Two Words Shown. Three Preparatory Words Spoken Leading to the First

Time — 50 seconds.
Characters — same as Table I.

Dog, Sheep, Horse. (1) Squirrel — Telegram

```
M.   | | | | | | | | | | | | | |
R.   | | | | | | | | | | | | | | |
K.   | | | | | | | | | | | | |
S.   | | — — — — — — — — — —
J.   | | | | | | | | | | | | | | | | | | |
Ht.  | | | | | | | | | | — — — — — — — —
```

(1) Dog, Sheep, Horse. Squirrel — Telegram.
(2) Pineapple, Banana, Orange. Fruit-Stand — Electric Light.
(3) Justice, Truth, Beauty. Charity — Photograph.
(4) Gold, Copper, Silver. Dime —Skull.

TABLE XI — *continued*

<table>
<tr><th></th><th>|</th><th>–</th><th>+</th><th>α</th><th>β</th></tr>
<tr><td rowspan="2">M.</td><td>28</td><td>3</td><td>15</td><td>0</td><td>8</td></tr>
<tr><td>51.8 %</td><td>5.5 %</td><td>27.9 %</td><td></td><td>14.8 %</td></tr>
<tr><td rowspan="2">R.</td><td>16</td><td>9</td><td>23</td><td>0</td><td>11</td></tr>
<tr><td>27.2 %</td><td>15.3 %</td><td>38.9 %</td><td></td><td>18.6 %</td></tr>
<tr><td rowspan="2">K.</td><td>56</td><td>2</td><td>0</td><td>0</td><td>0</td></tr>
<tr><td>96.5 %</td><td>3.5 %</td><td></td><td></td><td></td></tr>
<tr><td rowspan="2">S.</td><td>36</td><td>24</td><td>0</td><td>0</td><td>0</td></tr>
<tr><td>60.0 %</td><td>40.0 %</td><td></td><td></td><td></td></tr>
<tr><td rowspan="2">J.</td><td>38</td><td>4</td><td>24</td><td>1</td><td>7</td></tr>
<tr><td>51.4 %</td><td>5.4 %</td><td>32.4 %</td><td>1.4 %</td><td>9.4 %</td></tr>
<tr><td rowspan="2">Ht.</td><td>37</td><td>20</td><td>0</td><td>0</td><td>0</td></tr>
<tr><td>64.9 %</td><td>35.1 %</td><td></td><td></td><td></td></tr>
<tr><td rowspan="2">Totals</td><td>211</td><td>62</td><td>62</td><td>1</td><td>26</td></tr>
<tr><td>58.2 %</td><td>17.2 %</td><td>17.2 %</td><td>.2 %</td><td>7.2 %</td></tr>
</table>

Number of subjects, 6; number of sections, 4; number of experiments, 24; number of associations, 362.

TABLE XII. TWO WORDS SHOWN. THREE PREPARATORY WORDS SPOKEN LEADING TO NEITHER

Time — 50 seconds.
Characters — same as Table I.

Botany, Statue, Postal. (1) CHOCOLATE — BANNER

```
M.  | | – – – – – – – – – – – – – |
R.  – | | | | | | | | | | | | | | | | | | | |
K.  | – | | | – | – | – | – |
S.  | | – – – – – – – – – – – – – – – –
J.  | | | | | | | – – – – – – – – – – – – – – –
Ht. – – + + + – – – – – – – – – – –
```

(1) Botany, Statue, Postal. CHOCOLATE — BANNER.
(2) Idea, Proposition, Syllogism. SILVER — GAME.

<table>
<tr><th></th><th>|</th><th>–</th><th>+</th><th>α</th><th>β</th></tr>
<tr><td rowspan="2">M.</td><td>3</td><td>28</td><td>0</td><td>0</td><td>0</td></tr>
<tr><td>9.6 %</td><td>90.4 %</td><td></td><td></td><td></td></tr>
<tr><td rowspan="2">M.</td><td>22</td><td>1</td><td>2</td><td>0</td><td>14</td></tr>
<tr><td>56.5 %</td><td>2.5 %</td><td>5.2 %</td><td></td><td>35.8 %</td></tr>
<tr><td rowspan="2">K.</td><td>28</td><td>5</td><td>0</td><td>0</td><td>0</td></tr>
<tr><td>84.8 %</td><td>15.2 %</td><td></td><td></td><td></td></tr>
<tr><td rowspan="2">S.</td><td>13</td><td>20</td><td>0</td><td>0</td><td>0</td></tr>
<tr><td>89.4 %</td><td>60.6 %</td><td></td><td></td><td></td></tr>
</table>

TABLE XII — *continued*

J.	17 43.6 %	22 56.4 %	0	0	0
Ht.	0	14 48.3 %	8 27.6 %	7 24.1 %	0
Totals	83 40.6 %	90 44.2 %	10 4.9 %	7 3.4 %	14 6.9 %

Number of subjects, 6; number of sections, 2; number of experiments, 12; number of associations, 204.

TABLE XIII. TWO WORDS SHOWN. THREE PREPARATORY WORDS SPOKEN LEADING TO BOTH

Time — 50 seconds.
Characters — same as Table I.

Tree, Shrub, Grass. (1) TEA — FERN

M. + + *a a a a a a a* — —
R. + + + + + + + + + + + +
K. — — — — — — — — — — — — — — — —
S. — — — — — — — — — — — —
J. | —
Ht. + + *a a a a a a* | | | | |

(1) Tree, Shrub, Grass. TEA — FERN.
(2) Fox, Wolf, Moose. DEER — DOVE.

	|	—	+	*a*	*β*
M.	2 7.6 %	12 46.3 %	4 15.3 %	8 30.8 %	0
R.	0	0	25 100.0 %	0	0
K.	1 3.2 %	30 96.8 %	0	0	0
S.	8 33.3 %	12 50.0 %	0	4 16.7 %	0
J.	8 23.5 %	26 76.5 %	0	0	0
Ht.	8 28.5 %	12 42.8 %	2 7.2 %	6 21.5 %	0
Totals	27 16.1 %	92 54.8 %	31 18.4 %	18 10.7 %	0

Number of subjects, 6; number of sections, 2; number of experiments, 12; number of associations, 168.

TABLE XIV. TWO WORDS SHOWN (AUTHORS) — WITH A PREPARATORY SENTENCE LEADING TO THE SECOND

Time — 50 seconds.
Characters — same as Table I.

"God 's in His Heaven — all 's right with the world."

(1) BURNS — BROWNING

```
M.  + — — — — — — — — — | | | | | |
R.  | — — — — — — — — — — —
K.  + + + + + + + + + + + + +
S.  | — — — — — — — — — — — — — | | |
J.  | | — — — — — — — — — — — — — — — — — —
Ht. — — — — — — — — — —
```

(1) "God 's in His Heaven — all 's right with the world."
BURNS — BROWNING.

(2) "All that glistens is not gold."
BYRON — SHAKSPERE.

(3) "Hitch your wagon to a star."
SPENCER — EMERSON.

	|	—	+	α	β
M.	9 20.9 %	15 34.8 %	15 34.8 %	0	4 9.5 %
R.	1 2.1 %	47 97.9 %	0	0	0
K.	15 36.5 %	0	26 63.5 %	0	0
S.	6 15.0 %	34 85.0 %	0	0	0
J.	3 5.1 %	43 71.6 %	14 23.3 %	0	0
Ht.	0	18 58.1 %	13 41.9 %	0	0
Totals	34 12.9 %	157 59.7 %	68 25.8 %	0	4 1.6 %

Number of subjects, 6; number of sections, 3; number of experiments, 18; number of associations, 263.

Group VIII

The aim here is to test the effect of interruption in the series of associations, and to throw further light on the relation of the series to the present content of consciousness when this content is a series of associations and the new content is a pronounced word which is to act as a point of departure for new associations.

There are three divisions. The time in all is fifty seconds; in the first a word of general connotation is given and after an interim of ten seconds a second more specific word is pronounced. In the second three similar words are given at an interval of fifteen seconds; the third four words with an interval of ten seconds. The following are examples of the first and third divisions:

Subject III

Commencement — Sieve

commencement
| college ....commencement
| cap ....college
| gown.........cap
| boys ....commencement
| confetti.......commencement
sieve
— holes.........sieve
— water ....sieve
— flour ....sieve
— space ....holes
— concept.......space
— Royce........concept
— time ....concept
— eternity.......time
— damnation....eternity
— Hamlet ....."Consummation," etc
— Shakspere.....Hamlet

Subject VII

Wax — Jug — Tar — Sod

wax
| Charley................wax
| picnic..................Charley
| horse..................Charley
| saddle ....horse
jug
— ink....................jug
— clay...................jug
— Hegel ....jug
tar
/ 'old tar' ....tar
/ ship ....'old tar'
/ Bermuda..............'old tar'
sod
\ grave ....sod
\ graveyard ....grave
\ house ....graveyard
\ church ....house
\ music ....church
\ white ....church

In all the experiments the subject simply knew that possibly more than one starting-point would be given. There was of course the conscious recognition on the part of the subjects that the pronounced words were starting-points, which would imply an attentive consciousness, but they were cautioned neither to favor nor inhibit the newly pronounced word nor an association in progress.

The notes are uniform in showing that often one, two or three words of the former association-series are written after the new word is pronounced. "The momentum," says F., "was great enough to carry the associations two or three words beyond the pronounced word"; while Bl. found "a tendency for the trend of associations to persist, though not strong enough to overcome the new influence." By.'s experience was slightly different. As stated before, he often wrote the word as it came into consciousness. "On hearing a new word it gets precedence over the next associations not yet formed, and there is considerable confusion and lost time unless the motor discharge of writing the pronounced word is permitted to have free expression." The tables verify the same, and also show that there are more associations during the first interval.

Does a former starting-point regain its influence? In the first division there are two cases where the first and second fuse, but no place where the first independently forms an association; in the second but one word for subject Bl. in "Quill — Bench — Chalk," and in the third not any. There was a small amount of fusion in all, since but two words are due to the combined influence of the first and third, five to the combined influence of the first and fourth, with three starting-points, and one to the combined influence of the first and third with four starting-points.

The train of associations is inhibited by a new starting-point which dominates in influence. No mention is made in any note that a former starting-point remains in consciousness for the series, but M. emphatically writes, "Absolutely no influence of the preceding word or words when the next is taken up"; and later, "As soon as the new one is pronounced the old word and the series it had brought up were immediately suppressed." Bl. comments, "How remarkable it is that each new word crowds the old trend of associations out and starts new ones"; and the graphic representation, one of which only is given here on account of lack of space, shows that there is no return to the original series.

The tables are indicative of the tendency. In the first division of the group there are three possible lines of fusion, in the second six possible lines, and in the third twelve possible lines, but we find only 13.2 % for all forms of fusion in the first, 7.9 % for the second, and 10.5 % for the third. In the eighty-seven experiments of the series there is but one absolute return to the previous starting-point. (See Group IX, sec. 2, Bl.) The tables show the varying degrees of fusion, and while the percentages have little meaning, as there is a variable time-element,

the numbers do show accurately the number of words and the relative and continued influence of each starting-point.

We conclude that, when the present content of consciousness is a series of associations, the newly given impression establishes itself sufficiently to inhibit the associations of the previous series.

TABLE XV. TWO WORDS — GENERAL AND PARTICULAR — SPOKEN

Time — 50 seconds, with an interval of 10 seconds between 1 and 2.
Characters — same as Table I.

(1) Commencement — Sieve

M.	| | | — — — — — — — — — — — — — —
F.	| | | | | | + + + + + + +
H.	| | | | | — — — — — — — — — — — —
V.	| | | | | — — — — — — — — — — — —
S.	| | | | | | | | — — — —
E.	| | | — — — — — — — — —
Bs.	| | | — — — — — — — — — — — — — —
Ht.	| | — + + + + + +
By.	

	|	—	+	a	β
M.	3 17.6 %	14 82.4 %	o	o	o
F.	6 46.2 %	o	7 53.8 %	o	o
H.	5 29.4 %	12 70.6 %	o	o	o
V.	5 29.4 %	12 70.6 %	o	o	o
S.	8 66.6 %	4 33.4 %	o	o	o
E.	3 25.0 %	9 75.0 %	o	o	o
Bs.	3 17.6 %	14 82.4 %	o	o	o
Ht.	2 22.2 %	1 11.2 %	6 66.6 %	o	o
By.	o	o	o	o	o
Totals	35 31.4 %	66 55.4 %	13 13.2 %	o	o

Number of subjects, 9; number of groups, 1; number of experiments, 8; number of associations, 114.

Table XVI. Three Words Spoken

Time—50 seconds. Interval, 15 seconds.
Characters: | first, — second, / third, + partial fusion between first and second, α partial fusion between first and second with first predominating, β partial fusion between first and second with second predominating, γ partial fusion between first and third, δ partial fusion between second and third, ε partial fusion between first and third with third predominating.

(1) Gun—Bug—Jaw

```
M.   | | | | | | | | — — — — — — / / / / / /
F.   | | | | | — — — δ δ δ
H.   | | | | | | — — — — / / / / /
V.   | | | | | | | | — — — — — — / / / / / /
Bl.  | | | | | | — — — — — / / / / / / /
By.  | | | | | | | | | — — — — — — / / / / / /
Bs.  | | | | | + + + + / / / / /
Ht.  | | | | | | | α | | | δ — — — —
Ro.  | | | | | | — — — — — — / / / / / /
```

(1) Gun—Bug—Jaw
(2) Quill—Bench—Chalk
(3) Hall—Moss—Leather

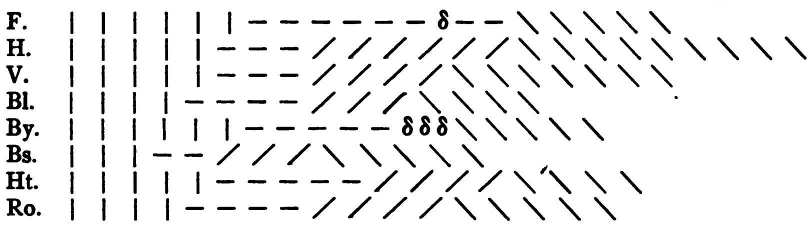
<table>
<tr><th></th><th>|</th><th>—</th><th>/</th><th>+</th><th>α</th><th>β</th><th>γ</th><th>δ</th><th>ε</th></tr>
<tr><td rowspan="2">M.</td><td>20</td><td>14</td><td>18</td><td>1</td><td>0</td><td>0</td><td>0</td><td>0</td><td>0</td></tr>
<tr><td>37.8 %</td><td>26.4 %</td><td>33.9 %</td><td>1.9 %</td><td></td><td></td><td></td><td></td><td></td></tr>
<tr><td rowspan="2">F.</td><td>15</td><td>11</td><td>15</td><td>0</td><td>0</td><td>0</td><td>2</td><td>3</td><td>0</td></tr>
<tr><td>32.6 %</td><td>23.9 %</td><td>32.6 %</td><td></td><td></td><td></td><td>4.4 %</td><td>6.5 %</td><td></td></tr>
<tr><td rowspan="2">H.</td><td>19</td><td>11</td><td>15</td><td>0</td><td>0</td><td>0</td><td>0</td><td>0</td><td>0</td></tr>
<tr><td>42.3 %</td><td>24.4 %</td><td>33.3 %</td><td></td><td></td><td></td><td></td><td></td><td></td></tr>
<tr><td rowspan="2">V.</td><td>24</td><td>15</td><td>21</td><td>1</td><td>0</td><td>4</td><td>0</td><td>0</td><td>0</td></tr>
<tr><td>36.9 %</td><td>23.1 %</td><td>32.3 %</td><td>1.5 %</td><td></td><td>6.2 %</td><td></td><td></td><td></td></tr>
<tr><td rowspan="2">Bl.</td><td>15</td><td>9</td><td>9</td><td>0</td><td>0</td><td>0</td><td>0</td><td>0</td><td>0</td></tr>
<tr><td>45.4 %</td><td>27.3 %</td><td>27.3 %</td><td></td><td></td><td></td><td></td><td></td><td></td></tr>
<tr><td rowspan="2">By.</td><td>23</td><td>8</td><td>14</td><td>3</td><td>0</td><td>4</td><td>0</td><td>3</td><td>0</td></tr>
<tr><td>41.8 %</td><td>14.5 %</td><td>25.5 %</td><td>5.5 %</td><td></td><td>7.2 %</td><td></td><td>5.5 %</td><td></td></tr>
<tr><td rowspan="2">Bs.</td><td>13</td><td>10</td><td>10</td><td>4</td><td>0</td><td>0</td><td>0</td><td>0</td><td>5</td></tr>
<tr><td>30.9 %</td><td>23.8 %</td><td>23.8 %</td><td>9.6 %</td><td></td><td></td><td></td><td></td><td>11.9 %</td></tr>
<tr><td rowspan="2">Ht.</td><td>23</td><td>11</td><td>10</td><td>1</td><td>1</td><td>2</td><td>0</td><td>1</td><td>0</td></tr>
<tr><td>46.9 %</td><td>22.5 %</td><td>20.5 %</td><td>2.0 %</td><td>2.0%</td><td>4.1 %</td><td></td><td>2.0 %</td><td></td></tr>
<tr><td rowspan="2">Ro.</td><td>15</td><td>15</td><td>24</td><td>0</td><td>0</td><td>0</td><td>0</td><td>0</td><td>0</td></tr>
<tr><td>27.8 %</td><td>27.8 %</td><td>44.4 %</td><td></td><td></td><td></td><td></td><td></td><td></td></tr>
<tr><td rowspan="2">Totals</td><td>167</td><td>104</td><td>136</td><td>10</td><td>1</td><td>10</td><td>2</td><td>7</td><td>5</td></tr>
<tr><td>37.8 %</td><td>23.5 %</td><td>30.8 %</td><td>2.3%</td><td>.2 %</td><td>2.3 %</td><td>.4 %</td><td>1.5 %</td><td>1.2 %</td></tr>
</table>

Number of subjects, 9; number of sections, 3; number of experiments, 27; number of associations, 442.

Table XVII. Four Words Spoken

Time—50 seconds. Interval, 10 seconds.
Characters — same as Table XVI, with addition of ╲ representing the fourth, ς partial fusion between first and fourth, η partial fusion between second and fourth, θ partial fusion between third and fourth, □ total fusion between first, second, third, and fourth, ι partial fusion between first, third, and fourth, κ partial fusion between second, third, and fourth, λ partial fusion between third and fourth, with fourth predominating, [partial fusion between first, second, and third.

(1) Den—Nag—Cot—Fan



(1) Den—Nag—Cot—Fan **(4) Bud—Car—Cub—Mat**
(2) Tax—Fan—Map—Dog **(5) Wax—Jug—Tar—Sod**
(3) Paw—Wand—Box—Mug **(6) Cur—Elk—Pug—Man**
(7) Rope—Wig—Ink—Grass

(*Table on page* 460)

Group IX

The aim here was to see if it were possible to have the first starting-point such that the conditions would be similar to the results obtained by using preparatory marginal settings, but include rather than inhibit the second starting-point. A review of the tendency toward mental combination in the former experiments suggested that the words be in the relation of the whole and part. The "part" was given the position of predominating influence in order to see to just what extent it would persist in combining.

In the light of the interpretation of previous facts and the subsequent results of this group of experiments, we are now in a position to conclude that, if the present content of consciousness on the reception of a new impression is such that the reactions are not antagonistic but reënforce each other, the second will not persist in independent influence,

	$\vert$	—	/	$\backslash$	+	γ	δ	s	η	θ	□	ι	β	κ	λ	[
F.	29	19	11	19	3	0	4	0	0	11	0	0	0	0	0	0
	30.3%	19.8%	11.4%	19.8%	3.1%		4.2%			11.4%						
H.	31	18	24	25	3	0	0	0	0	8	0	0	0	0	0	0
	28.5%	16.5%	22.1%	22.9%	2.7%					73%						
V.	28	28	21	50	0	0	0	0	0	0	0	0	0	0	0	0
	22.0%	22.0%	16.6%	39.4%												
Bl.	18	17	17	29	0	0	0	0	0	0	0	0	0	2	0	0
	21.6%	20.5%	20.5%	34.9%										2.5%		
By.	36	22	9	24	2	4	3	0	6	2	4	0	0	0	0	0
	32.1%	19.6%	8.0%	21.4%	1.8%	3.7%	2.6%		5.3%	1.8%	3.7%					
Bs.	22	13	17	18	4	1	0	1	0	6	0	3	1	0	5	0
	24.2%	14.3%	18.7%	19.8%	4.4%	1.1%		1.1%		6.6%		3.3%	1.1%	0 %	5.4%	
Ht.	30	18	10	21	3	0	0	0	2	1	0	0	0	0	0	2
	34.4%	20.6%	11.4%	24.2%	3.4%				2.4%	1.2%						2.4%
Ro.	24	22	16	48	0	0	3	0	0	0	0	0	0	0	0	0
	21.3%	19.4%	14.2%	42.4%			2.7%									
Totals	218	157	125	234	15	5	10	1	8	28	4	3	1	2	5	2
	26.6%	19.2%	15.2%	28.5%	1.9%	.6%	1.3%	.2%	.9%	3.4%	.4%	.3%	.2%	.3%	.7%	.3%

Number of subjects, 8; number of sections, 7; number of experiments, 52; number of associations, 819.

but will be rather included in and supplementary to the influence of the first, which otherwise would be less assertive. The results below show that while the first of the two starting-points has a decided disadvantage of position, and therefore has little influence in arousing associations, it here is responsible for 43.2 % and the second for but 9.5 %, while there is a combined influence of 47.3 %, the first strongly predominating in partial fusion. There is but one case where the second was followed exclusively.

The explanation of this, extended to an hypothesis, would rest in the fact that each word has definite characteristic reactions and that the fusion of two words or lines of thought means that the motor accompaniments are such that they unite and reënforce each other, or that the one includes the other. There are few antagonistic impulses.

Table XVIII

Two Words — Whole and Part — Spoken

Time — 15 seconds.
Characters — same as Table I.

(1) Crowd — Man

<table>
<tr><td>A.</td><td>| + +</td></tr>
<tr><td>M.</td><td>+ + + + + + + + +</td></tr>
<tr><td>F.</td><td>+ + + + +</td></tr>
<tr><td>H.</td><td></td></tr>
<tr><td>J.</td><td>| | | | | | + + +</td></tr>
<tr><td>S.</td><td>— — — —</td></tr>
<tr><td>V.</td><td>+ + + a a a a a a</td></tr>
<tr><td>E.</td><td>| + + +</td></tr>
<tr><td>L.</td><td>| | | |</td></tr>
<tr><td>Bs.</td><td>a a a a a a</td></tr>
<tr><td>Br.</td><td>| | | | | + +</td></tr>
</table>

(1) Crowd — Man
(2) Ton — Pound
(3) Moose — Horn
(4) Engine — Whistle
(5) Book — Page
(6) Music — Octave

<table>
<tr><th></th><th>|</th><th>—</th><th>+</th><th>a</th><th>β</th></tr>
<tr><td rowspan="2">A.</td><td>4</td><td>3</td><td>24</td><td>0</td><td>0</td></tr>
<tr><td>12.9 %</td><td>9.6 %</td><td>77.5 %</td><td></td><td></td></tr>
<tr><td rowspan="2">M.</td><td>13</td><td>0</td><td>27</td><td>2</td><td>0</td></tr>
<tr><td>30.9 %</td><td></td><td>64.2%</td><td>4.9 %</td><td></td></tr>
<tr><td rowspan="2">F.</td><td>7</td><td>3</td><td>22</td><td>1</td><td>0</td></tr>
<tr><td>20.6 %</td><td>8.8 %</td><td>64.8 %</td><td>5.8 %</td><td></td></tr>
</table>

TABLE XVIII — *continued*

H.	28 73.6 %	5 13.2 %	0	0	5 13.2 %
J.	21 46.6 %	1 2.3 %	17 37.8 %	6 13.3 %	0
S.	25 67.5 %	12 32.5 %	0	0	0
V.	4 12.5 %	2 6.3 %	16 50.1 %	10 31.1 %	0
E.	11 39.2 %	3 10.8 %	9 32.2 %	5 17.8 %	0
L.	14 58.4 %	6 25.0 %	4 16.6 %	0	0
Bs.	15 44.2 %	2 5.8 %	11 32.4 %	6 17.6 %	0
Br.	25 58.2 %	0	9 20.9 %	9 20.9 %	0
Totals	167 43.2 %	37 9.5 %	139 35.9 %	39 10.0 %	5 1.4 %

Number of subjects, 11; number of sections, 6; number of experiments, 62; number of associations, 387.

GROUP X

In the last group the words were called whole and part, although the reader no doubt observed that this was not always strictly the case, but rather that the more complex was more influential as a word-suggester than the simple and often tended to include it. Can this be demonstrated in another way? Attention was called in Group of Experiments IV (two pictures shown) that picture no. 3 (a man sawing wood), although first, had more than the normal number of associations, the explanation of which might lie in the fact that it contained more objects. We here attempt to test this by comparing a comparatively complex and a comparatively simple picture as starting-points. It has been clearly proven that the second picture to be presented has a decided advantage of position. The simple picture is here given the advantage of position. The notes show the first to be more important, while there is no suggestion of the first including the second, on account of there being less fusion than for words and more independent associations for the second starting-point; the averages are 38.5 % for the first, 30.5 % for the second, 31.0 % fusion.

(a) Picture of Blacksmith shoeing horse
(b) Picture of a Sheep.

| shop....................(a)
| nails....................shop
| shoe ....shop and (a)
| blacksmith ....shop and shoe and (a)
| Longfellow ....blacksmith
— pasture ....(b)
— sheep ....pasture
— lambs ....pasture and sheep
— grass ....pasture and lambs
— hillsides ....grass
— brook....................pasture and hillsides
| iron ....shop-shoe
| hammering ....iron
| soldier ....iron-hammering and first group
| battle ....soldier
| shoe ....battle
| horse-shoe nail ....iron, hammering, soldier, battle
| anvil ....horse-shoe nail

Table XIX. Two Pictures Shown

Time — 50 seconds.
Characters — same as Table I.

(1) (a) Blacksmith shoeing Horse
(b) A Sheep

J. | | | | | — — — — — — — | | | | | | |
E. | + + — — | | |
W. | — | — — — | | | | | | |
V. | | + + + + + + + + + + + + +
L. — | | | | | | | |
Ht. — — *a* *a* | | | | — + + + + +

(1) (a) Blacksmith shoeing Horse
(b) A Sheep
(2) (a) Girl and Boy
(b) A Bird
(3) (a) Three Children
(b) A Duck
(4) (a) A Sower
(b) A Dog

	$\mid$	—	+	*a*	*β*
J.	30	23	9	18	0
	37.5 %	28.8 %	11.2 %	22.5 %	
E.	28	10	2	0	0
	70.0 %	25.0 %	5.0 %		
W.	14	18	5	0	0
	37.8 %	48.7 %	13.5 %		
V.	6	11	37	0	13
	8.9 %	16.4 %	55.3 %		19.4 %

TABLE XIX — *continued*

L.	27	4	1		
	84.3 %	12.5 %	3.2 %	0	0
Ht.	16	30	6	6	0
	27.6 %	51.8 %	10.3 %	10.3 %	
Totals	121	96	60	24	13
	38.5 %	30.5 %	19.2 %	7.6 %	4.2 %

Number of subjects, 6; number of sections, 4; number of experiments, 24; number of associations, 314.

GROUP XI

Three words spoken, one immediately following the other, are given in this group in order to test the span of consciousness and the influence of an immediate interruption, as one was given immediately following the other in such a way that all associations were checked until after the third starting-point. If we attempt to follow the initial starting-points we see they may disappear after the first few associations and reappear in the series; or each remain as initial starting-points for a few associations; or one alone control the whole series, while the others are present without influence; or there may be an alternation of independent influences; or an influence which shows a modifying effect of one or both of the other starting-points, which may reach such a degree that all fuse, and in so doing get an advantage over the single words. The starting-points are to a large extent disparate and there is very little fusion; we find only .5 % total fusion and 18.7 % partial fusion.

An example would be as follows :

SUBJECT VIII

FROG — ICE — TABLE

— snow ....ice
| bench ....table
| fish............frog
| pole ....fish
| boat ....fish
| line ....fish
| bait ....fish
| weeds ....fish
— skating ....ice
— sleds ....ice
— coasting........sleds
— girls ....coasting

There were no cases of the total fusion of all the starting-points throughout the series, no cases of total fusion of the first and second, and two cases for the second and third. There were two cases of partial fusion of the first, second, and third throughout. The first was followed exclusively in two cases, the third in one. The first and third were followed intermittently in four cases, the second and third in eleven, the first, second, and third in thirty-five.

The second starting-point has the ascendency of influence. This is not due to habits formed in earlier experiments, as the two groups of experiments with two words were given in connection with other experiments. This group informs us that in immediate interruption the new impression has not sufficient power to establish itself more firmly than a past impression which is just past, and must in a manner be reproduced to start associations. The influence of the first has been somewhat destroyed, but the second is greater than the third.

Table XX. Three Words Spoken

Time — 50 seconds.

Characters — same as Table XVI, with the addition of [representing total fusion between the first, second, and third, μ partial fusion between first, second, and third with first predominating, ν partial fusion between first, second, and third with first and third predominating.

(1) Frog — Ice — Table

A. / — — — — —
M. — | | r r r r r r r r r
F. — — — — — / / | | | | / / / — — —
H. | — — — — — | | | | / / / — — —
J. | — — / / / / — + + + + + + + +
Bl. — / | | | | | | — — — —
By. | / / / / / / — — — — — — — — — — — — — — — —
Bs. — | | | — — — / /
Ro. | | — — / / / / / / / / / / /
Br. — / / / δ δ δ δ δ δ

(1) Frog — Ice — Table
(2) Key — Shoe — Knife
(3) Desk — Park — Glove
(4) College — Church — Cafe
(5) Boston — Elevator — Lake
(6) Skate — Book — Theatre
(7) Bridge — Sleigh — Ticket
(8) Gun — Lamp — Watch

Table XX — *continued*

<table>
<tr><th></th><th>|</th><th>—</th><th>/</th><th>+</th><th>α</th><th>β</th><th>γ</th><th>δ</th><th>[</th><th>μ</th><th>ν</th></tr>
<tr><td>A.</td><td>26
26 %</td><td>32
32 %</td><td>30
30 %</td><td>0</td><td>0</td><td>0</td><td>0</td><td>0</td><td>2
2%</td><td>10
10%</td><td>0</td></tr>
<tr><td>M.</td><td>25
24.3%</td><td>36
34.9%</td><td>21
20.5%</td><td>3
2.9%</td><td>0</td><td>0</td><td>10
9.8%</td><td>1
.9 %</td><td>1
.9%</td><td>0</td><td>6
5.8%</td></tr>
<tr><td>F.</td><td>15
15.9%</td><td>23
24.5%</td><td>24
25.5%</td><td>8
8.5 %</td><td>11
11.8%</td><td>0</td><td>7
7.4%</td><td>6
6.4%</td><td>0</td><td>0</td><td>0</td></tr>
<tr><td>H.</td><td>31
23.8%</td><td>68
52.4%</td><td>31
23.8 %</td><td>0</td><td>0</td><td>0</td><td>0</td><td>0</td><td>0</td><td>0</td><td>0</td></tr>
<tr><td>J.</td><td>19
15.5%</td><td>18
14.6%</td><td>48
39.1 %</td><td>15
12.2%</td><td>0</td><td>0</td><td>0</td><td>22
17.8%</td><td>1
.8%</td><td>0</td><td>0</td></tr>
<tr><td>Bl.</td><td>23
23.2%</td><td>33
33.2%</td><td>16
16.0 %</td><td>5
5.0 %</td><td>0</td><td>0</td><td>0</td><td>22
22.6%</td><td>0</td><td>0</td><td>0</td></tr>
<tr><td>By.</td><td>13
8.7%</td><td>56
36.8%</td><td>48
31.5%</td><td>15
9.8 %</td><td>0</td><td>1
.6%</td><td>17
11.2%</td><td>2
1.4%</td><td>0</td><td>0</td><td>0</td></tr>
<tr><td>Bs.</td><td>19
23.4%</td><td>13
16.0 %</td><td>19
23.4%</td><td>9
11.2%</td><td>0</td><td>0</td><td>0</td><td>19
23.5 %</td><td>2
2.5%</td><td>0</td><td>0</td></tr>
<tr><td>Ro.</td><td>32
32.6%</td><td>33
33.7%</td><td>33
33.7%</td><td>0</td><td>0</td><td>0</td><td>0</td><td>0</td><td>0</td><td>0</td><td>0</td></tr>
<tr><td>Br.</td><td>12
15.0%</td><td>24
30.0%</td><td>38
47.5%</td><td>0</td><td>0</td><td>0</td><td>6
7.5%</td><td>0</td><td>0</td><td>0</td><td>0</td></tr>
<tr><td>Totals</td><td>215
20.3%</td><td>336
31.1%</td><td>308
29.1%</td><td>55
5.2%</td><td>11
1.1 %</td><td>1
.9%</td><td>40
3.6%</td><td>72
6.8%</td><td>6
.5%</td><td>10
.9%</td><td>6
.5%</td></tr>
</table>

Number of subjects, 10; number of sections, 8; number of experiments, 79; number of associations, 1060.

Group XII

In order to test the relative influence and to throw more light on the problem of immediate interruption four words were shown and the subject was directed to read from left to right. There were few experiments and most of the subjects were new. The results show that the third starting-point has a decided disadvantage, having an influence of only 7.8 %. The second and fourth are almost equal, while the first is again less.

Table XXI. Four Words Shown

Time — 50 seconds.
Characters — same as Table XVII, with the addition of ξ partial fusion between the second, third, and fourth with the second predominating.

(1) Gun — Car — Ink — Fan

M. \ \ \ — — — — — / / / / / /

R. \ \ \ \ \ \ \ \ \ \ \ \ \ \ \ \ \

K. \ \ \ \ \ \ \ \ \ \ \ \ \ \ \ \

S. — | / — — — — — — — —

J. | | | — — — — — — — — — — —

Ht. + + a a a | | | | | | | | |

TABLE XXI — *continued*

(1) Gun — Car — Ink — Fan
(2) Brain — Umbrella — Telephone — Chain
(3) Book — Money — Hour — Chart

	$\vert$	—	/	$\backslash$	+	γ	θ	α	κ	ξ
M.	14 35.9%	6 15.4%	9 23.0%	4 10.3%	0	6 15.4%	0	0	0	0
R.	16 34.8%	0	0	30 65.2%	0	0	0	0	0	0
K.	0	35 68.6%	0	16 31.4%	0	0	0	0	0	0
S.	5 16.2%	15 48.4%	7 22.5%	4 12.9%	0	0	0	0	0	0
J.	13 27.7%	13 27.7%	4 8.5%	15 31.9%	2 4.2%	0	0	0	0	0
Ht.	15 34.8%	9 20.9%	0	5 11.6%	2 4.7%	0	7 16.3%	3 6.9%	1 2.4%	1 2.4%
Totals	63 24.5%	78 30.4%	20 7.8%	74 28.5%	4 1.6%	6 2.4%	7 2.6%	3 1.4%	1 .4%	1 .4%

Number of subjects, 6; number of sections, 3; number of experiments, 18; number of associations, 257.

GROUP XIII

Four similar words were pronounced in immediate succession, the results of which show that we are correct in calling the above a case of succession and also to establish clearly and definitely that there is a difference of degree between immediate and postponed interruption. The third starting-point has again a decided disadvantage of influence and exerts no apparent influence in thirty-seven of the fifty-two experiments. The last starting-point exerts the greatest influence.

TABLE XXII. FOUR WORDS SPOKEN

Time — 50 seconds.
Characters — same as Table XVII with the addition of o partial fusion between first, second, and third with first and second predominating.

(1) Cow — Roof — Fence — Girl
(2) Cathedral — River — Elevator — Newspaper
(3) Cane — Harness — Box — Coat
(4) Book — Snow — Rope — Stone
(5) Wire — Flower — Horse — Paper
(6) Gun — Wharf — Chair — Stove

	|	—	\	/	+	γ	δ	η	θ	□	ι	ο	ε	β
M.	35 33.4%	3 2.8%	6 5.8%	32 30.4%	0	3 2.8%	17 16.2%	0	9 8.6%	0	0	0	0	0
F.	16 21.3%	11 14.6%	1 1.4%	35 46.6%	0	3 4.0%	0	0	1 1.4%	2 2.7%	1 1.4%	0	5 6.6%	0
H.	21 20.6%	38 37.3%	11 10.8%	30 29.5%	0	0	0	0	0	0	0	1 .9%	0	1 .9%
V.	16 19.3%	16 19.3%	0	36 43.3%	2 2.4%	2 2.4%	9 10.9%	0	1 1.2%	0	1 1.2%	0	0	0
Bl.	36 46.8%	8 10.3%	10 12.9%	19 24.8%	0	0	4 5.2%	0	0	0	0	0	0	0
By.	23 24.5%	30 31.9%	3 3.3%	36 38.3%	1 1.0%	0	0	0	1 1.0%	0	0	0	0	0
Bs.	22 33.3%	14 21.3%	1 1.6%	20 30.0%	1 1.6%	0	0	7 10.6%	0	1 1.6%	0	0	0	0
Ht.	9 11.5%	19 24.3%	4 5.2%	31 39.8%	0	0	0	0	1 1.3%	14 17.9%	0	0	0	0
Ro.	26 36.6%	23 32.4%	6 8.4%	16 22.6%	0	0	0	0	0	0	0	0	0	0
Totals	204 27.2%	162 21.6%	42 5.4%	255 33.9%	4 .5%	8 1.1%	30 3.9%	7 .9%	13 1.8%	17 2.3%	2 .3%	1 .2%	5 .7%	1 .2%

Number of subjects, 9; number of sections, 6; number of experiments, 52; number of associations, 751.

We ask finally how far our results and notes point to a theoretical understanding of the mechanism of associations. Previous work, especially that of James, Cordes, Calkins, and Scripture, as well as the accumulated notes of my subjects, confirm that the transition may be made by means of total, partial, and focal recall, and that in partial and focal recall the prominent persisting elements are surrounded in the formation of a new idea by other new elements.

If a latent idea remains in the margin of consciousness and exerts an influence, which not merely modifies but determines the series of associations, and leads up to the focalisation of the latent idea, we have a case of predetermined association, which, when noted by investigators, has invariably become confused with mediate association. Here there is an element or group of elements, persisting in the margin of consciousness, which is gradually maturing and becoming focalised into groups of elements comprising an idea which ultimately dominates consciousness. In some cases three, four, and five ideas have been named before this takes place, and we have here a reversed form of association. Four subjects noted the experience on different occasions, and it is not to be confused with the common experience of apprehending the present contents of consciousness as part of a larger whole where we are conscious of its existence but not of what it is.

The notes further show that the common conscious elements may be predominantly visual, auditory, olfactory, gustatory, or kinæsthetic, or a complex or compound of these in character, while to this may be added an indication of the fact that the transition, incipient as it is, may in many cases be reduced to a condition which is in the last analysis one of the motor nervous system. Ht., for instance, finds that the words all pass over into innervations of the organs of speech and "are accompanied by the impulse to make the sound," stating later, "they hang on the tongue." The following is one of the series given which represents rather an extreme case, Taft, taffy, toffy; tough, rough, ruff; buff, bluff, tough; muff, duff, tuff. Br., who also gave a large percentage of verbal associations, finds that "some part of each word seems to linger on the tongue with motor sensations till the next comes." "I am subject," he adds, "more or less frequently to verbal automatism of this auditory incipient motor type." Ro., who has many auditory associations, reports "they are always accompanied by motor images, together with many associations." A changing of orientation is a common accompaniment, with statements of the feeling of the impulse to turn in various directions. For F., who is predominantly of the motor type, we have an example where the rhythmic ticking of

a clock fades into the rhythmic watching of a boat rising and falling on the water.

The notes would seem to indicate that there is no idea without a motor fringe, and also that these elements of incipient impulses to movement may accompany the elements of transition, and are observed introspectively by the subjects. They are therefore data for psychology. Do they influence or direct the associations? In short, are they the processes which connect and which determine the associations?

F. states, "There seem to have been waves of motor sensations. Such waves may start with a word and carry one in faint mimicry through the whole succession of bodily sensations that one experienced in that event, and then may come a relapse until other stronger currents appear." Here we are face to face with the dynamics of association, the most fundamental and important problem of brain association. Have these phenomena of ideational images "acquired by contact a kind of magnetism which causes the one to attract the other and have, so to speak, become magnetic?" (Zanotti.) Or are they on the other hand independent of all force and "merely ideas of antecedence and sequence only?" (Mill.) While there is no mention of a magnetic force, the notes and results all show that the ideas are systematically conditioned in a way which cannot be explained by the contiguity of the objects. The motor elements play the deciding role. Ht. emphasises the influence of ideated movement when he writes, "Kinæsthetic. Slow regular tramping on snowshoes brought up the characteristic swing of arms, and therewith the idea (sensations of weight) of the stick (or stock) which I have generally carried on Norwegian snowshoes. Transition from Vermont to the Black Forest by association with snowshoeing in both places. Real sensations in play were free breath, movements in chest (kinæsthetic), fresh air (olfactory), cold (thermal), and emotion of emotional strength." Again, "Looking up at sun suggested general ideas of expansion of attention and with this breath comes the idea, breezes"; another subject adds, "A tendency to imitate the sounds of syllables and this leads on to a train of associations"; another, "A slight feeling of sudden changed impulse"; another, "A sort of motor after-image came back and took the foreground"; and F. goes further when he states, "Ideationally my hand wandered to the upper right-hand corner of the page, then suddenly the auditory image of 47 came up as if whispered to me." All of which indicate that some ideas at least depend for their entrance into consciousness upon motor reactions.

Passing to the more refined reactions expressed in emotions we find that they are not merely accompanying coloring influences, but also often actual determining factors. All of the subjects notice at some time a coloring atmosphere from an emotion, but others find that "the growing word is rather felt emotionally than definitely formulated," and we have "a nameless idea, largely feeling-tone" (Ht.); or the words may "all come as parts of a growing feeling, an indistinct though strong state of mind." (J.) The same subject observed, "The previous word may create a mood or feeling which in the main determines the associations; a group of words is dependent upon strong accompanying feeling — there is a summation and a discharge while the next word has been accumulating force" (J.), and we have a form of summation; or in other words, "a general mood accumulated while several words were in mind at once, then all dropped and another general feeling came to the front with an accumulation of other words." (F.) Here we have a typical example of constellation where all the words and ideas are implicitly present as a total attitude or disposition, the elements of which become successively focalised into a series of associated images. The last subject finds that "the emotional atmosphere often controls the associations." Indeed, it would seem that occasionally for some subjects this strong accompanying undercurrent of undifferentiated emotional feeling is capable of bringing about trains of thought independent of any logical connection. K. finds "the feeling to carry one on"; H. finds the "point of departure the interesting idea"; all find that the words change with the disposition, as may be verified by a study of the lists of associations.

We are forced to conclude that the impulses to movement or other emotional attitudes may act as determining factors in association, which extended to an hypothesis would mean that the mode of transition in the associated series is in the last analysis to be found in delicate incipient motor tendencies to action, the psychic concomitants of which are observable; that psychic states are both as to their unity and organisation consequences of motor reactions which are implicitly present as parts of a total reaction to the present situation. It is these motor tendencies to action which determine what idea shall enter consciousness. Just in so far as they become released they become prolonged, accentuated, and form a nucleus for the new idea. To speak of association independent of motor elements is merely to make an empirical classification of successive states of consciousness.

There remains a psychical phenomenon which must be satisfactorily

accounted for before we go farther. An element of an idea, an idea or a series of ideas may occupy consciousness to the exclusion of others. If the second starting-point were not given, the associations would undoubtedly follow the given one. Inhibition must then be one form of "obstructed association," the inhibiting ideas being present to the exclusion of the inhibited. But are we thus forced to say inhibition is the "negative side of the association process," claiming that all ideas not in consciousness are inhibited, and thus being forced to conclude the conscious idea is inhibiting an unconscious idea, which cannot exist (by the very definition and presuppositions of psychology) until it is an object of consciousness. This would mean that content of consciousness and inhibition are identical. On the other hand, the notes and exemplifying facts of the tables show Dr. Breese's fallacious position when he concludes that "because, obeying the laws of association, the train of ideas takes one direction rather than another can hardly be considered sufficient ground to hold that the other possible train of ideas is inhibited." [1] He has overlooked the possibility of two or more trains of associations having been started and the associations of one starting-point are excluded from entering the focus of consciousness by the direction of the given series. Inhibition would then be the negative side of fusion. The explanation must, as has already been demonstrated, be psycho-physical in character. If these impulses to action have actually been observed by the subjects we are justified in concluding that just as in physiological inhibition one action excludes another, so the correlative tendencies to movement of one idea exclude others.

By. observed that the image of the starting-point lingered and inhibited subsequent ideas. The implication here, from our previous reasoning, would be that not the ideational images, as such, but the physiological motor concomitants, persisted and excluded others, and this is why disparate terms give a "shock to the nervous system" (A.), "require different lines of expression" (A.); and "one has more momentum," as so many report. This would explain why the associations of a new starting-point inhibit the associations of a former one; for as the motor nervous impulses tend to work themselves out into action, the reaction of the previous impulse will be suppressed by those of a new impulse which enters, by the conditions of these experiments, an attentive consciousness. Thus the prepotent impulses to action are the conditioning factors in mental inhibition.

All this indicates that the basis of habit which has been the universal principle of explanation of associations is inadequate. As

[1] Breese, B. B.: On Inhibition, Psy. Rev. Monograph, vol. 3, p. 15.

Münsterberg has pointed out, contrary to what we mean by habit, either idea may bring to consciousness the other, in a manner independent of the order of the original presentation. Extending our hypothesis to include the formation of associations, the conclusion will be that in order for two ideas to become associated they must be together in consciousness, each as parts of a total experience, a total attitude; the motor reactions of the ideas must be parts of a more comprehensive reaction which includes both as simultaneous correlated motor impulses: when, in future time, the reactions of the one are reëxperienced, there is a sequence of infinitely delicate and complex impulses to movement, and any tendency toward such reaction tends to reproduce the whole of which it is a part, as each reaction is more or less bound up in the integrity of the whole central nervous system.

DISSOCIATION

BY C. H. TOLL

THE purpose of this investigation, of which the following gives a preliminary report, was to compare the tendency to associate by contiguity, with the tendency to associate by similarity.

In every series of stimuli to which one gives attention there is tendency to association by contiguity. But some similarity among certain elements of the series may produce a dissociation of the given elements into two series with some bond of similarity in each. This is a matter of common experience, as when you find you can read your newspaper and listen to your neighbors' conversation at the same time, understanding both, although the actual order in which the several words are perceived would form a meaningless mixture.

We may say dissociation is always accomplished by a tendency to association by similarity overcoming the constant tendency to association by contiguity. Study of the relative efficacy of the two may therefore be called a study of dissociation. The tendency to associate by contiguity might be measured in two ways.

First, when one attempts to learn a series in exactly the given order, the number of errors in the series as recollected may be taken as an inverse indication of the strength of association by contiguity. The three kinds of error possible in nearly all of the experiments were Omissions, Displacements, and Imperfections. All of these three have been tabulated. But the number of elements omitted seems considerably the most reliable as an indication of the degree of inadequacy of the associative tendency. The cases of displaced or imperfect elements are comparatively few: moreover, Displacements and Imperfections are not mutually exclusive categories. A single element may be both imperfectly recollected and wrongly placed in the recollected series. On the whole, it seems that the number of given elements which were omitted in the recalled series is the most positive and reliable of the errors. Our conclusions are based on the Omissions.

Second, when one makes no attempt to learn the series, simply giving attention to each element as it comes, and afterward lets the elements recur spontaneously, the number of cases in which a recollected element is followed by an element given contiguously may be taken as a direct indication of the strength of association by contiguity.

Tendency to association by similarity can evidently be measured in the same two ways, by counting errors when one purposes to learn the series as two groups of similar elements, and by counting sequences of similar elements when one avoids any effort to learn the series and recollection is spontaneous.

In the first seven experiments we used the first method. The errors made when the purpose is to associate by contiguity can then be compared with the errors made when the purpose is to associate by similarity, an equal number of series, given under the same conditions, and of identical character, being given in each case.

In the last four experiments we have used the second method. The number of sequences of elements given contiguously can then be compared with the number of sequences of similar elements.

Five subjects have coöperated in this, but the experiments were strictly individual, one observer being alone in the room with the experimenter. Each test lasted about an hour. As a matter of course, the results have been calculated for each of the five subjects and their agreements and disagreements have been carefully considered. But as this first report is to indicate merely the general tendency, we give here at first only the average of the five persons.

The experiments have varied as to the kind of elements used, the manner of presentation, the time allowed, and the manner of recording the recollected series. But throughout each experiment the series were of one identical type, while the individual elements were altered in each series.

In the experiments where the series were to be learned, some in the given order, some dissociated by similarity, it was found rather confusing to turn from one method to the other; so several consecutive series were learned by one method, and then several by the other, four alternations being made each hour to neutralize any effect of practice or of fatigue.

The series were of course different in kind in the several experiments, but were usually of eight or of ten elements. Half of this number had some distinct characteristic in common, the other half some other characteristic. In some experiments these elements were alternated, in some arranged irregularly.

In the first eight experiments the subject wrote down the elements recalled, as soon as the series had been given. In the last three the subject spoke the elements recalled.

In all cases where the first method of measurement was used, the time allowed for learning the series was made a little too short to permit of learning the series perfectly. Since comparison of the number of mistakes was our method, we naturally had to make sure there would be mistakes to compare.

The details of the several experiments were as follows:

(1) The elements were letters and numbers. They were about 12×8 mm. in size and were printed on white cards 15×30 mm.

Five letters and five numbers were placed, alternately, in a straight row on a sheet of white cardboard. The series was then exposed to the subject by turning up the small tin shutter of a screen that was clamped to the table-edge.

The time of exposure was measured with a stop-watch and was constant throughout the hour for each individual subject. Four seconds proved the best time for most of them, but in one case it was necessary to allow only three seconds. Twenty series were presented during each hour, ten for each method of memorizing. There were duplicates of all the numbers, and of eight letters, but not more than two of any element. Selection in forming the series was by chance. In dissociating, the letters were separated from the numbers.

As soon as the exposure was ended, the subject wrote down the elements recollected, trying to preserve their relative order. This recollected list was then copied beside the operator's record of the given series, so making the errors apparent.

(2) The elements were all letters, printed as before, and the alternate cards were placed half their length out of alignment with the original row.

The method of presentation, the length of exposure, the number of elements presented, etc., were as in no. 1. In dissociating, the letters on one level were separated from those on the other.

(3) The elements were all letters, printed as before, and five of the ten elements presented were placed out of alignment. But the disaligned cards were at irregular intervals and often in groups, and were only a quarter of an inch out of alignment. This order was varied each time, but without any system.

The other details were as in no. 1.

During this experiment I came to notice the effect produced by the natural tendency to learn the five elements of the dissociated series

in a rhythmical form, thereby increasing the ability to retain them; while there appeared to be no natural tendency to apply any such inclusive rhythm to the ten elements of the series when learned in the given order. To counteract this effect the subjects were instructed to consider the series, when learned in the given order, as two consecutive series of five elements each, and to use the same natural rhythm in learning these as they did in the dissociating. But this correction was not made in the first two hours, nor very perfectly in the rest.

(4) The elements were all numbers, printed as before, five of the ten being placed a quarter of an inch out of alignment, and in irregular groups, precisely as in the last experiment.

The time was reduced to three seconds for some and two seconds for the others. Details of presentation were as described in no. 1.

This time all the subjects tried to neutralize the effect of the instinctive rhythm for the five-element series by learning the ten-element series in two groups of five elements each.

(5) The elements were all nonsense syllables, each consisting of a vowel between two consonants, printed on white cards 20 × 20 mm. Eight of these were placed in an even row on a sheet of white cardboard, and four of them were marked by laying a quarter-inch strip of blue paper over the bottom of the card. The serial position of the marked cards was irregular, and was altered each time.

Ten seconds was given to some subjects, eight to the others. Other details of exposure, etc., were as in no. 1.

In learning the series in the given order, the blue markings were ignored; but in dissociating, the marked and unmarked syllables were learned in separate groups.

There seemed to be no rhythmical tendency; but to be safe the subjects were instructed to learn the straight series in groups of fours.

Seven series were given to be learned in each method during the hour with each subject.

(6) The elements were one-syllable nouns, alternated with nonsense syllables, all spoken by the operator. The nonsense syllables were all different from those used in the preceding experiment: the nouns were ordinary words, and were so arranged as to avoid any obvious sequence or relation among them. Very few, if any, were used twice in one hour. Five nouns and five syllables were given in each series.

The elements were spoken at the rate of forty-six a minute, timed by a metronome which was muffled in a heavily padded box so that

its sound was no disturbing factor. The speaker sat within three feet of the subject and enunciated as distinctly as possible.

Dissociation was performed as previously: in each hour eight series were dissociated, and eight learned in the given order.

(7) The elements were one-syllable nouns, spoken as before, alternated with nonsense syllables, printed on small white cards. The nouns were all different from those used in the previous experiment: the nonsense syllables were the same, but were this time printed, in letters 10 mm. high, on cards 40 mm. square. They were exposed by sliding them, one at a time, in front of an opening in a cardboard screen which was fastened to the table-edge.

The optimum rate for presenting the elements was found to be about forty a minute, measured with the metronome.

Five nouns and five nonsense syllables were given in each series. Eight series were given to be learned in the given order, and eight to be dissociated into separate series of nouns and of syllables.

(8) The elements were names of mammals, alternated with names of cities of the United States, all spoken. The names were all fairly familiar. Ten elements were given in each series.

The interval in reading was planned to be long enough for some appreciation of the meaning of the words, but not enough to permit mental repetition of the preceding elements. Any mechanical time-measurement was found impracticable.

The subjects were instructed to avoid any effort to memorize the series, simply receiving each element as given.

After the last element there was a pause of about two seconds, to decrease the mere sound-recollection of the last few elements. Then the operator repeated, in an altered tone, one of the given elements. The subject at once wrote down the first element that came to mind, then the next, and so on.

In the seven preceding experiments the set of series presented had been different for each subject, though of course identical in character. But in this experiment and the following ones the lists of words were identical as read to each subject. The same element was repeated for each. Sixteen lists were given.

(9) The elements were nouns. In each series five names of similar objects were alternated with five names of a different sort of objects, *e. g.*, names of fishes with names of poets. All were read, as before. In each series new sorts of objects were chosen. The subject never knew what sort of words were to be given; the subjects agreed this was not a disturbing factor to them, and it obviated the tendency

to think what words would probably be given, as is natural when the general character of the series is announced beforehand.

The subjects were instructed to be passive during the reading, and during the four-second pause that followed, avoiding mental repetition of the words. Then the operator gave a signal and the subject repeated aloud the words as they happened to be remembered. The words being numbered on the list from which they were read, the operator was able to record the words as fast as spoken.

The subjects were instructed to give the word which they found to be foremost after they had spoken the preceding one, rather than to try to repeat a group of words which usually appeared simultaneously at the first effort of recollection, but which usually faded while one of them was being spoken.

The same sixteen series, of ten elements each, were given to each subject.

(10) The elements were nouns, the ten presented in each series all being names of similar objects, *e. g.*, flowers. Five were spoken, alternated with five printed on small cards which were shoved in front of a 10 × 10 cm. opening in a cardboard screen fastened to the table-edge. Cards were 40 mm. square, the words printed by hand, but carefully, in letters 10 mm. high.

A series was given in about 13 seconds, but the time was not mechanically measured; it was at a rate which some practice showed to give a fair time to comprehend each element.

As before, the subjects were told to be passive until, after a four-second pause at the end of the series, the operator gave a signal. Then the recollected words were spoken.

The class of nouns was different in each series.

(11) The elements were nouns. In each series five of some familiar class were alternated with five of some other familiar class. The classes were different in each of the twelve series given.

From this regular series of ten, five were chosen irregularly, and were printed on cards as in no. 9. The remaining five, of course also irregularly placed in the series, were spoken. This irregularity was different in each series. Thus some words of one kind were spoken, the rest printed; some words of the other kind were spoken, the rest printed.

The other conditions were exactly as in the last experiment.

A table for the individual subjects, indicating not only the omitted but also the displaced and imperfect objects would have, for instance,

the following character: C indicates that the effort was made to associate by Contiguity, S by Similarity.

SPOKEN NOUNS, ALTERNATED WITH PRINTED NONSENSE SYLLABLES

	Nouns Omitted		Syll. Omitted		Displaced		Imperfect	
	C	S	C	S	C	S	C	S
Turley	13	16	21	14	7	13	6	10
Emerson	4	5	26	16	7	4	4	13
Miss Kent	5	8	15	8	18	5	9	4
Flexner	4	6	10	9	7	3	8	16
Toll	8	7	8	2	10	12	8	3
Total	34	42	80	49	49	37	35	46

If we consider total results only, and among them only the omitted elements, we come to the following percentages. They give the percentage of the errors of omissions among the elements recalled.

1. Letters and numbers alternated	C 26.	S 10.8
2. Letters, alternatingly disaligned	C 21.2	S 15.
3. Letters irregularly disaligned	C 23.8	S 22.4
4. Numbers irregularly disaligned	C 7.	S 20.
5. Nonsense Syllables, irregularly marked	C 27.5	S 27.5
6. Nouns and Nonsense Syllables alternated, spoken	C 35.	S 37.2
7. Nouns and Nonsense Syllables alternated, nouns spoken, syllables printed	C 28.5	S 22.7

In the second group, experiments 8 to 11, not the errors of omission, but, as explained above, the different kinds of reproduced elements, had to be analyzed with special reference to the question whether a sequence linked two contiguous or two similar objects. In the following table the total number of recalled sequences is taken as basis and the different kinds of sequences are given in percentages of it. The elements themselves are described above. B means a break, that is, a sequence without similarity or contiguity.

8. Dissimilar elements, similarly presented	S 45	C 28	B 28
9. Dissimilar elements, different kind in each series	S 53	C 25	B 21
10. Similar elements, dissimilarly presented	S 54	C 20	B 26
11. Dissimilar elements, dissimilarly presented	S (Meaning) 27	C 7	B 8
	S (Presentation) 13.		

The results by the first method of measurement may be summarized as follows, though the first and third conclusions are weakened by disagreement among the individual subjects.

A. When the only dissociating factor is some slight unessential

MOTOR IMPULSES

feature (a bit of color on the card, a slight disalignment), this similarity and contiguity are nearly equally efficient. No. 3 and no. 5.

As this unessential feature is made more striking (disalignment half a card-length), the strength of similarity increases, only three fourths as many errors being made in dissociation as in contiguous association. No. 2.

The case of no. 4 (all numbers) is of little or no value. The time allowed for learning had to be made short enough to ensure the appearance of some errors; perfect recollection would obviously give no basis for comparison. And the time had to be so short in this case (only two seconds for some of the subjects) that the additional eye-motions and adjustments necessary in dissociating took time enough to spoil the results.

B. When the only dissociating factor is in the meaning of the elements (letters and numbers), this similarity is stronger than contiguity, only one half as many errors being made. No. 1.

The results of no. 6 do not support this proportion, but its results are not consistent, while those of no. 1 are.

C. When both meaning and manner of presentation are combined as dissociating factors (nouns and nonsense syllables, seen and heard), this similarity is stronger than contiguity, only three fourths as many errors being made.

But this method of measurement is not well adapted to series of auditory elements, so this experiment is unsatisfactory. No. 7.

The results by the second method of measurement may be summarized as follows:

A. When the only dissociating factor is in the meaning of the elements (names of different sorts of objects), this similarity is stronger than contiguity, twice as many similarity sequences as contiguity sequences being recalled. No. 8 and no. 9.

B. When the only dissociating factor is in the manner of presentation (to sight and hearing), this similarity is stronger than contiguity, nearly three times as many similarity sequences being recalled. No. 10.

C. When both meaning and manner of presentation are dissociating factors, these similarities are much stronger than contiguity, more than four times as many similarity sequences being recalled.

D. When these two dissociating factors are opposed to each other: (1) Four of the subjects show similarity of meaning much stronger than similarity of presentation, from two to five times as many similarity-of-meaning sequences being recalled. (2) One subject is strongly and consistently otherwise, giving nearly three times as many similarity-of-presentation sequences. No. 11.

MOTOR IMPULSES

THE ACCURACY OF LINEAR MOVEMENT

BY B. A. LENFEST

THE starting-point for our investigation was the observation of Woodworth[1] that there is a certain rhythm in which a certain hand-movement is made with the maximum of exactitude, and which represents thus an optimum for the periodical discharge of the particular motor çentre. Our question was whether this rhythm is a constant one for all parts of the body, or whether different groups of muscles produce the greatest exactitude in different periods; further, whether secondary factors, like complexity of movement, resistance by weight, fatigue, etc., influence this psycho-physiological optimum.

The investigation, however, showed soon the necessity to consider the whole problem of the accuracy of rhythmical linear movements, and the experiments are thus not always directly related to our starting-point.

There is very little material published that can be collected under the subject head, accuracy of voluntary movement, and still less when the enquiry is confined to the accuracy of straight lines or linear movements.

The most suggestive contribution is that of Dr. Woodworth on the accuracy of voluntary movement. He has collected consistently what can be found up to the date of his publication, and the reader is referred to pages 7–16 of his monograph for the most reliable collection of authorities.

It must be said, as we run over the list from Goldscheider on the threshold of perceptible movement, through the results of Hall, Hartwell, Loeb, and Delabarre on "bilateral asymmetry" and comparisons of right and left hands; consider Fullerton and Cattell in their suggestive results, and Münsterberg's studies of movements; and finally take the testimony of Bryan as to the growth of accuracy of

[1] Woodworth: The Accuracy of Voluntary Movements, Psychological Review Monographs, no. 13, 1899.

movement in children, that the vast accumulation of material bearing on reaction time-and similar phenomena would be of more value if concerned more with the accuracy and less with the production or perception of movement.

A paper by Miss M. K. Smith, in the Philosophische Studien for 1900, with the title, Rhythmus und Arbeit, concerns the influence of rhythmical action upon the quality and quantity of work performed. The method was to commit to memory nonsense syllables and letters.

The results show a tendency to take up a certain rhythm, especially in the later results and after practice; easier memorizing if rhythm is present; motor reactions, as tapping, nodding, or swaying of body are noted frequently; the feeling of pleasure accompanies rhythmic reactions. While there are no data as to accuracy, there is suggestive matter bearing on the optimal rate and on the relations of compound and simple movements of the hand.

As far as the writer knows, he is the first to present systematic results as to the head and foot movement. The purposes of this enquiry may be briefly stated as

(1) the collection of a large body of facts, bearing on the actual and relative accuracy of straight-line movements possible with various parts of the body, such as hands, arms, head, legs, and feet;

(Something like 340,000 lines have been drawn and calculated.)

(2) to introduce certain variations in the conditions attending the production of ruled lines, such as

(*a*) to rule with the eyes opened and eyes closed, with other conditions the same;

(*b*) to change the rate of ruling or interval between the production of ruled lines; the rates chosen were 20, 30, 40, 50, 60, 70, 80, 100, 120, 140, 160, 180, and 200 beats per minute;

(*c*) to change the length of the normal or first line; the lengths used were 14, 10, and 1 cm.;

(*d*) to impose a weight on the ruling hand to either retard or accelerate the movement, choosing a weight of such magnitude that it would be perceptible, but would not have mass enough to cause pain or fatigue; 260 grams was used;

(*e*) to introduce a simultaneous movement of the free hand; *i. e.*, the one that did not carry the recording pencil, of a similar character and extent but of opposite direction to the ruling hand;

(*f*) to record movements of both hands, of the head and of both feet;

(*g*) to conduct a series of experiments of similar character, as regards time-rate and extent of movement, to the series presented by Dr. Woodworth, with the idea of corroborating or disproving the results of his investigations; lines of 140 cm. were accordingly chosen;

(*h*) to conduct a series of experiments where the subject chooses his own rhythm or rate at which the easiest and best lines, subjectively speaking, could be ruled;

(*i*) to find the rates of respiration and pulse-beats and find the connection, if any, between them and the linear records.

(3) To examine, by variations of the number of lines ruled, the questions of fatigue and persistence of the memory-image; series of 50 lines for the first year and of 20 lines for the second year, were accordingly selected.

(4) To find the relations, if any, between constant errors and mean variations, so called.

The Apparatus

It is proposed to give the briefest possible discussion or explanation of the apparatus required for the investigation, it being desired at a later stage to enter into a comparison of the method adopted here with that of the only other investigation at all comparable to this one: the research problem of Dr. Woodworth, already referred to.

The underlying principle has been to avoid complication in apparatus, partly because of the delay and expense involved in working out, and making up elaborate schemes for apparatus, but mainly because of the advantage in duplicating this series of experiments, or of carrying on related investigations, to be derived from a choice of such parts, entering into the complete apparatus, as are at hand in any psychological laboratory, or that can be obtained and set up at small expense.

The use of smoked paper has been avoided, because a short preliminary series, using the usual smoked-paper records, was found to give no better results than did the method here adopted of ruling on white paper with a soft pencil, and the labor was thus considerably reduced.

To the objection that the pencil-ruling is more difficult, and involves more loss in friction and more complicated adjustments on the part of the subjects, only one of fourteen subjects admits that this is the case; and even if the testimony was unanimous as to the greater ease of production of the smoked records, it would be no

reason for its adoption, since one of the first rules for all experimental work is uniformity of conditions, and this is equally well attained in either case.

The apparatus for free hand-movements and for the compound movements of both hands consists:

(1) Of an adjustable wooden rest (see Fig. *A*) with a base (*a*) about 40 x 60 cm. hinged to a vertically adjustable flat board (*b*), called the arm-rest, about 40 x 70 cm., and having on its upper edge two brass pins or plates (*c*) about 30 cm. apart.

The pencil is started from one of these pins, depending on the hand used, and moved until it comes in contact with a wooden rod that is held against the opposite pin and which is of the right length to give a movement of the pencil of 1, 10, or 14 cm., as desired.

The operator holds this rod in place for the first line ruled and then instantly removes it, so that the second and all later lines are ruled by memory of the first one, as closely in length to the first, or so-called normal line, as is possible.

(2) The apparatus for actuating and taking care of the paper.

This consists of two drums (*d* and *d'*, Fig. *B*) 20 cm. diameter by 40 cm. wide, mounted on suitable supports about 1 metre apart, and fastened to a table, with axes parallel.

The drum upon which the record is to be made (*d*) is adjusted close to the arm-rest, so that each ruled line will be carried down and out of sight before the next one is ruled, the pencil being held in the position (*e*); note that the arrow shows the direction of rotation.

The second drum (*d'*) is actuated by a motor (*F*) through a round belt (*g*), this motor being a clockwork type, with gear-changes and adjustable vanes for varying the speed,and having the power derived from a suspended weight (*w*).

The recording paper (*h*) transmits motion from (*d'*) to (*d*). This paper consists of a strip about six metres long by twenty-eight cm. wide, with one end pasted to (*d*), and then wound upon (*d*), leaving enough to be carried to (*d'*) and pasted to the latter. As the paper is unwound from (*d*), it is wound upon (*d'*), and, both to keep the paper tight and to prevent too rapid unwinding of (*d*), it is necessary to apply a friction-brake to the shaft of (*d*).

(3) A metronome, capable of being used for a range of 20 to 200 beats, and a stop-watch, to enable the operator correctly to time the subject, are in constant use.

The metronome is set in vibration and the subject is permitted to take his own time to start the ruling, the operator holding the

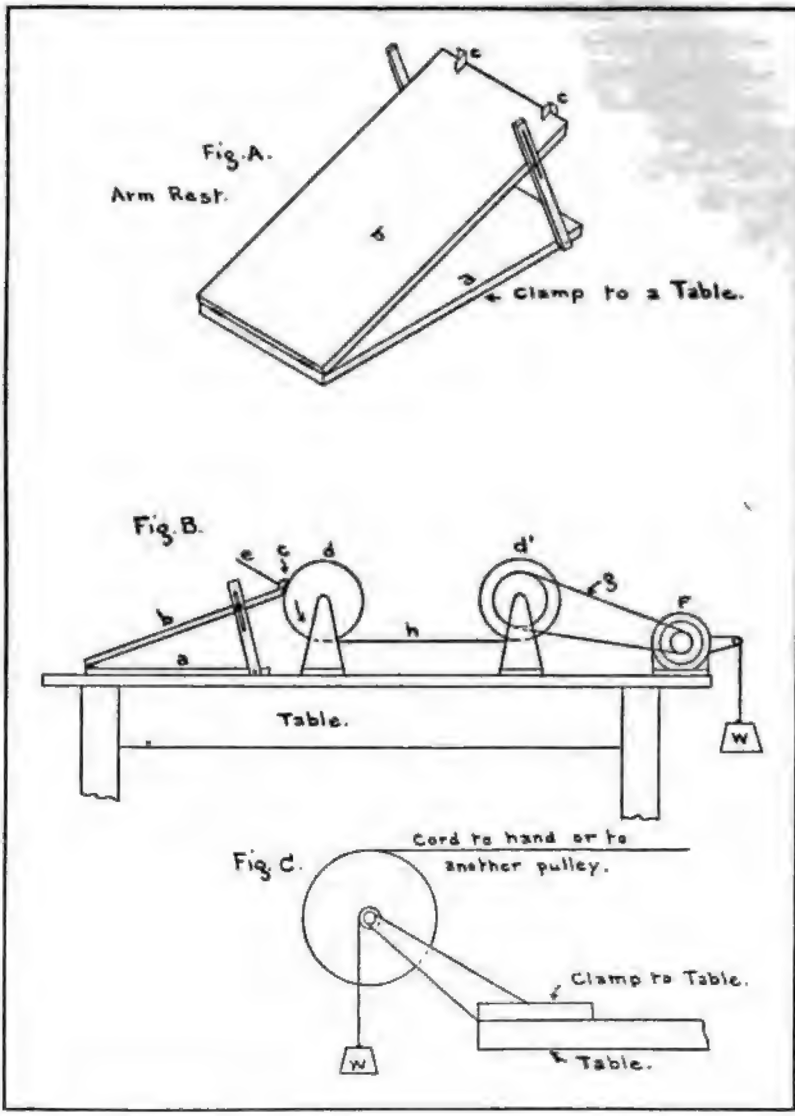


Fig. A.
Arm Rest.
Clamp to a Table.
Fig. B.
e
c
d
d'
g
F
b
a
h
Table.
W
Fig. C.
Cord to hand or to another pulley.
Clamp to Table.
W
Table.

wooden rod in place with one hand, while the other hand holds the stop-watch ready to start it the instant the subject's pencil is moved. There is thus a personal equation for the length of period, but this is of no consequence, as will be apparent when the method of calculation and the use of the planimeter is considered.

In the series of records with the weight, it is impossible to run the speed about 80 to 100 beats, unless the modification in apparatus shown in Fig. *C* is used; for the vibration of the string running from the hand to the weight around a pulley is violent enough either to throw the string off the pulley or cause the weight to jump so severely as to render the records useless.

This is entirely obviated by the given method of using a heavy weight acting with a small leverage (about 1 cm.) and thus moving only a short distance, so that it is capable of operating at the highest speeds with no perceptible shock or jump; the string is led to the hand or wrist from a grooved pulley of about 12 cm. radius, so the highest velocity of the weight is only about one twelfth that of the hand. This method makes it possible to carry the weighted records to the highest speeds.

This same method is used for the head and foot records, with the following additional apparatus; the string (Fig. *C*), shown leading to the hand, is led horizontally over to and around a similar large pulley on the opposite side of the table and either down to the foot or in a diagonally upward direction to the head; so that movements of the head or foot are faithfully recorded on the drum by means of a pencil held in a block of wood, this block of wood being fastened on the horizontal string in a suitable position for recording on the drum paper. The pencil is kept against the paper by a light spring or elastic band.

The foot is connected to the string by a stirrup that prevents any movement of the feet at all, unless the same is recorded by the pencil.

The head is furnished with a skull cap or harness consisting of non-elastic webbing and stiffened, where the string is attached, by a strip of sheet brass formed to fit the forehead or the back of the head, as the case may be. The object of the brass strip is to prevent a lost motion in the flexible webbing, that is found troublesome otherwise.

It will be evident, then, that the weight is continually acting as an accelerating or retarding influence in all records for head and feet, but it is not considered objectionable, for it is a constant throughout the series.

The other plan would require a circuit of cord leading in both

directions from the head or feet in a complete circuit, and would cause in the opinion of the writer too much complication of apparatus.

The pulse-beats were taken by the stop-watch and wrist method so familiar to the physician, while the respiration results were obtained by the usual tambour apparatus for registering the chest expansion upon smoked paper.

The Method of Calculation

Suppose that the drums have been set in rotation and that the paper is unwinding from (*d*) and being wound on (*d'*), Fig. *B*, and suppose that the subject has ruled series of 20 to 50 lines, as may be desired, regulated by the stop-watch in the hands of the operator. The records will appear much as Fig. 5 under the planimeter discussion, there being for each speed one normal line to start and a series of lines following and intended to be of the same length as the normal line. A series of records, then, consists of 13 records of 20 or 50 lines, each running from 20 to 200 beats per minute, the complete series having not less than 260 and not more than 650 lines.

It should be added that the operator holds a pencil-point on the end of each normal line just after the record of 20 or 50 lines is made and turns the drum (*d*), thus marking a line nearly perpendicular to the ruled lines and at the average or normal distance from the starting-point; an absolutely correct record would show all ruled lines ending on this line.

The calculation of this series of records by the ordinary method of measuring each line, adding the lines of the series, averaging for the constant error, and repeating the operation in a slightly different form for the mean error or mean variation is of such enormous labor for an extended investigation as to be beyond the capacity of one or of several students; it is fortunate that the planimeter is at hand to be employed in averaging each series, and this instrument has therefore been selected as overcoming this difficulty.

It is desirable to consider the method employed by Dr. Woodworth to overcome this danger of excessive computation, and it will now be subjected to a critical and comparative examination.

He says, page 19 of his monograph on the Accuracy of Voluntary Movement, that the subject's sole duty was to make the present line equal to that immediately preceding, and the width of the slot was so adjusted that the subject could see only the line just ruled. After discussing certain matters of memory and its relation to the memory-image, in the attempt to support this changing normal plan, he con-

fesses, on page 20, that this device is advantageous in much simplifying the most tedious part of the graphic method, that of computation.

While this is undoubtedly true, it needs careful scrutiny before adoption, for, on the same page, he says that one source of error in the method of making each line equal to the preceding one is that the different movements in the same series are not comparable, but the positive constant error is cumulative in its effect, and the normal tends to become longer and longer.

Some relation between this source of error and such a record as shown on page 29, Fig. 2, is evident, for, while it should be noted that this cumulative effect is peculiar to a series of lines for one speed, it has further a tendency to produce overruling at all speeds, and the natural result is to increase the error unduly and unnaturally for the higher speeds or as the speed increases, because there is then less time for the discrimination and choice that will tend to shorten the ruled line. It may be predicted, then, that Dr. Woodworth's method will show a slight lengthening of normal between lines at slow speeds and a much greater one at high speeds, the effect being to introduce a variable factor that would have no existence were a better plan adopted. The computation required for the average error is simple, being dependent only on the first and last lines of a series, and it is suspected that this very simplicity has led to its adoption and the consequent neglect of certain serious sources of error.

He tells us, on page 20, that the constant and variable error may well be isolated and studied separately, but indicates that they must "somehow" be considered combined as nature has made them; that is, analysis is desirable, but the synthetic method is more scientific.

This investigation will present data suggesting that

(1) Such a curve as that on page 29 of his monograph is not a characteristic one and relations of length of ruled line, as well as effects of weight, make it impossible to apply Weber's law or even the law of Fullerton and Cattell in the way proposed by Dr. Woodworth.

(2) There is no relation, mathematical or other, between constant and mean errors, and they not only may be but must be isolated and studied separately, if an investigation is to be conducted in the interests of scientific exactness.

It will be necessary to reject the method of Dr. Woodworth if the most reliable results are desired, in which case the planimeter is a necessity.

The theory of the planimeter cannot be developed at this place; every physicist and engineer is acquainted with it. The writer believes

he was the first to apply the planimeter to the calculation of results from psycho-physical data for averaging both mean and variable errors. More than 340,000 lines were involved, each demanding two measurements. The best type of planimeter for general use and the one used here is the Amsler adjustable-arm form.

In Fig. *D* is shown a record taken at twenty beats per minute that

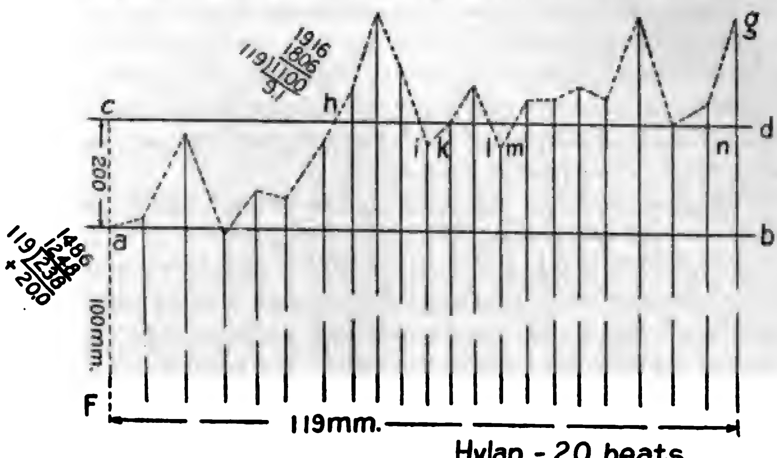


Fig. D.

will both explain the method of computation and show how the planimeter has been used to find the constant and mean errors.

The record, as made and ready for computation, is not provided with the line *cd* or with the dotted lines that connect the ends of the ruled lines. The line *ab* is drawn by turning the drum of the apparatus with a pencil held at the end of the normal or left-hand line *af*, which was here 100 mm. long.

The tracing-point of the planimeter being placed at *a*, a reading is taken, which was in this case 1486; after following with the tracing-point the dotted path to *g* and returning, via *gb* and *ba*, a second reading is taken, which was 1248; subtracting gives 238, which should be read 2380 square mm. for the area of the space *agba*; dividing by the distance *ab*, in this case 119 mm., gives the average height, which is +20.0 mm., the plus sign suggesting that the distance thus found, which is the constant error for the series, be laid off in addition to or beyond *Fa*.

This being done, a line *cd* is drawn parallel to and 20.0 mm. from *ab*, as the mean line of constant errors.

To find the mean error of the series a slightly different method is necessary.

Place the tracing-point of the planimeter at *c* and read vernier, giving 1916; follow the dotted path from *c* to *h*, the straight line from *h* to *i*, the dotted path from *i* to *k*, the straight line from *k* to *l*, the dotted path from *l* to *m*, the straight lines from *m* to *n* and *n* to *g*, the dotted path from *g* to *m*, the straight line from *m* to *l*, the dotted path from *l* to *k*, the straight line from *k* to *i*, the dotted path from *i* to *n*, and the straight line from *h* to *c*, when a second reading is taken, which was in this case, 1806. Divide the difference of these two readings, 1100 mm., by the length of *cd*, 119 mm., and the result is 9.1 mm., or the mean error (mean variation).

It will be noted that this method gives the sum of the errors from the mean line *cd*; that is, the same result would be obtained if the tracing-point were (1) carried from *c* around all the area below *cd*, and this area were calculated as before; (2) carried from *c* around all the area above *cd* and the area measured as in other cases; and (3) these two results added and averaged.

To apply the method for *ab*, or constant error computation, to *cd* should give equal readings at *c* or a 0 mean error, a result evidently incorrect in the record selected.

After averaging results by the planimeter, the collection of data has been arranged by months; the record for one month only can be presented here, but the method of tabulation is the same throughout.

Each figure given for N, M, c and v, in the accompanying typical table for the month of May, 1904 (pages 495–499), is the average from 20 or 50 lines, ruled as already shown, Fig. *D*.

Results

It is necessary to observe that the limits of space imposed on the writer preclude all but the barest outline of the deductions to be drawn from the investigation, and to this fact is due whatever of dogmatism is inherent in the argument; for it is manifestly impossible to present all the material, and the writer asks, then, the indulgence of the reader when he claims to have impartially examined and presented the evidence.

Hand Movements

Simple movements
Lines 14 cm. long.

Typical Series for the Month

Key.
v = mean error. R.H. = right hand. R.F. = right foot. E.O. = eyes open. si. = simple motion
N = normal line. Unit = 1 mm. L.H. = left hand. L.F. = left foot. E.C. = eyes closed. co. = compound motion.
M = mean line. b = beats per minute.
c = constant error.

Day.	Subject.	See Key.	Beats per minute. 20	30	40	50	60	70	80	100	120	140	160	180	200
6	Hylan.	N	10	10.5	11	11	10	10	10	12	10	12	10	11	11
	L.F.E.O.	M	16.1	13.5	12.1	13.8	13.1	10.0	10.0	11.2	12.5	12.0	6.1	11.0	14.2
		c	+6.1	+3.0	+1.1	+2.8	+3.1	0.0	0.0	−0.8	+2.5	0.0	−3.9	0.0	+3.2
		v	2.8	3.5	1.9	0.9	1.6	4.6	2.0	2.7	1.2	1.0	1.3	2.1	4.0
	Hylan.	N	10	11	10	12	10	12	11	11	11	11	11.5	11	11
	L.F.E.C.	M	8.9	10.5	10.9	11.4	13.1	10.3	11.8	13.1	13.9	15.8	11.0	15.0	17.8
		c	−1.1	−0.5	+0.9	−0.6	+3.1	−1.3	+0.8	+2.1	+2.9	+4.8	−0.5	+4.0	+6.8
		v	2.7	6.4	2.2	1.4	2.7	1.6	2.3	1.6	4.9	2.9	2.3	2.3	2.5
	George.	N	10.5	10	10	10	10	10	10	10	11	10	10	11	11
	L.F.E.O.	M	14.6	9.7	7.1	6.4	7.7	8.3	8.0	10.0	11.0	9.0	10.0	8.0	12.5
		c	+4.1	−0.3	−2.9	−3.6	−2.3	−1.7	−2.0	0.0	0.0	−1.0	0.0	−2.0	+1.5
		v	2.1	1.6	1.8	2.3	0.8	0.7	1.4	1.7	0.8	1.0	1.3	1.6	0.4
	George.	N	10	9	9	10	11	9.5	9	10	8	10	9	9	10
	L.F.E.C.	M	11.7	9.2	10.2	12.6	13.2	11.5	12.2	6.6	5.0	8.0	7.6	8.0	11.0
		c	+1.7	+0.2	+1.2	+2.6	+2.2	+2.0	+3.2	−3.4	−3.0	−2.0	−1.4	−1.0	+1.0
		v	2.0	2.9	2.5	0.7	0.5	1.0	0.8	2.0	4.1	3.1	2.5	2.1	3.7
	Moore.	N	10	10	10	10	11	11	11	10	10	10	11	10	10
	L.F.E.O.	M	15.7	18.8	17.7	16.7	18.3	18.5	16.5	15.6	16.0	16.2	16.3	16.4	18.8
		c	+5.7	+8.8	+7.7	+6.7	+7.3	+7.5	+5.5	+5.6	+6.0	+6.2	+5.3	+6.4	+3.8
		v	3.5	3.8	1.1	0.8	3.6	2.1	2.7	0.6	2.4	0.5	3.5	2.2	3.8

															140 beats
Moore. L.F.E.C.	N	10	10	10	10	11	10	10	9	9.5	11	11	10	10.5	
	M	19.6	15.3	15.3	14.3	14.9	13.5	6.7	13.4	20.0	14.3	14.5	16.9	17.9	
	c	+9.6	+5.3	+5.3	+4.3	+3.9	+3.5	−3.3	+4.4	+10.5	+3.3	+3.5	+6.9	+7.4	
	v	2.6	2.5	2.4	0.4	2.0	6.4	0.7	4.1	2.6	3.8	1.1	3.1	2.8	
9 Angier. R.F.E.O.	N	10	10	9	10	10	9.5	10	10	11	10	10	10	10	
	M	13.6	12.5	11.6	11.6	11.5	11.7	13.0	12.9	12.2	12.2	13.5	22.1	16.5	
	c	+3.6	+2.5	+2.6	+1.6	+1.5	+2.2	+3.0	+2.9	+1.2	+2.2	+3.5	+12.1	+6.5	
	v	2.9	2.3	1.8	2.3	1.7	1.6	2.0	2.7	3.3	1.5	1.5	6.7	1.5	
Angier R.F.E.C.	N	10	10	10	9.5	10	9	10	10	9	10	10	11	10	10
	M	11.4	7.7	13.8	8.3	11.4	11.5	11.0	10.3	9.9	12.8	16.7	17.7	12.4	12.0
	c	+1.4	−2.3	+3.8	−1.2	+1.4	+2.5	+1.0	+0.3	+0.9	+2.8	+6.7	+6.7	+2.4	+2.0
	v	4.8	1.3	3.0	2.6	1.4	1.7	1.8	1.7	1.8	1.4	1.7	4.2	2.1	1.7
Huggins. R.F.E.O.	N	11	10	10	9	10	11	10	10	10	11	11	11	10	
	M	12.5	8.5	12.6	9.7	9.7	16.6	15.7	18.7	13.6	15.8	9.3	18.3	14.3	
	c	+1.5	−1.5	+2.6	+0.7	−0.3	+5.6	+5.7	+8.7	+3.6	+4.8	−0.7	+7.3	+4.3	
	v	3.6	2.6	2.7	2.8	3.3	3.2	3.3	3.6	3.1	2.7	2.9	3.8	3.0	
Huggins. R.F.E.C.	N	11	8	10	10	11	10	10	10	10.5	10	10	10	10	
	M	6.5	10.5	10.7	8.8	12.6	12.9	12.2	22.3	9.9	21.8	12.0	12.5	16.3	
	c	−4.5	+2.5	+0.7	−1.2	+1.6	+2.9	+2.2	+12.3	−0.6	+11.8	+2.0	+2.5	+6.3	
	v	1.4	2.1	1.8	1.6	3.7	3.1	2.0	4.1	1.4	5.0	2.3	3.6	1.8	
13 Lenfest. R.F.E.O.	N	9	9	9	9	9.5	8	7	10	10	10	9	8	10	
	M	12.7	11.9	11.1	11.3	12.1	8.7	11.6	12.6	9.0	8.6	11.7	7.0	11.8	
	c	+3.7	+2.9	+2.1	+2.3	+2.6	+0.7	+4.6	+2.6	−1.0	−1.4	+2.7	−1.0	+1.8	
	v	3.7	2.4	1.9	2.3	2.8	0.2	2.8	2.1	2.0	2.1	3.2	1.4	4.1	
Lenfest. R.F.E.C.	N	12	11	11	10	10	10.5	11	11	11	11	10	10	10	
	M	14.9	14.7	12.2	13.5	12.6	11.9	12.6	8.7	9.1	8.9	8.4	14.1	6.9	
	c	+2.9	+3.7	+1.2	+3.5	+2.6	+1.4	+1.6	−2.3	−1.9	−3.1	−1.6	+4.1	−3.1	
	v	3.3	2.2	3.5	3.5	3.2	3.0	2.6	1.7	2.4	2.8	2.9	4.1	2.3	

	N	12	11	11	11	11	10	11	10	11	10.5	11	10	10
George.	M	8.3	9.0	8.6	8.8	8.7	7.0	7.4	7.9	6.8	9.7	9.0	5.8	11.0
R.F.E.O.	c	−3.7	−2.0	−2.4	−2.2	−3.3	−3.0	−3.6	−2.1	−4.2	−0.8	−2.0	−4.2	+1.0
	v	5.0	3.3	2.3	0.1	3.3	2.9	2.4	3.1	1.5	4.6	2.9	2.9	3.9
	N	12	11	12	11	11	11	10.5	11	10	10.5	11	10	10
George.	M	7.0	7.7	14.1	8.3	10.3	8.4	8.7	7.8	6.4	10.0	9.0	7.9	7.3
R.F.E.C.	c	−5.0	−3.3	+2.1	−2.7	−0.7	−2.6	−1.8	−3.2	−3.6	−0.5	−2.0	−2.1	−2.7
	v	2.5	2.6	2.4	3.7	2.9	2.3	1.6	2.1	1.9	3.7	2.9	1.8	3.8
	N	11	11	11	11	12	10	9	11	10	10.0	10.5	11	9.5
Moore.	M	14.5	16.6	16.5	9.0	17.0	10.7	10.4	11.9	11.5	12.5	12.5	15.1	12.6
R.F.E.O.	c	+3.5	+5.6	+5.5	−2.0	+5.0	+0.7	+1.4	+0.9	+1.5	+2.5	+2.0	+4.1	+3.1
	v	1.7	2.2	1.9	1.8	1.3	2.2	2.6	2.2	2.2	2.5	2.5	1.4	0.8
	N	11	10	11	11	11	10	10	9	11	11.0	10	10	10
Moore.	M	14.3	13.2	15.9	12.3	17.9	10.0	11.3	15.9	14.8	15.5	13.4	11.8	12.2
R.F.E.C.	c	+3.3	+3.2	+4.9	+1.3	+6.9	0.0	+1.3	+6.9	+3.8	+4.5	+3.4	+1.8	+2.2
	v	1.8	2.8	1.6	2.1	1.7	2.4	3.2	2.2	2.7	1.4	2.2	1.8	2.2
	N	11	11	10	11	10	10	10	11	10	11	10	11	11
16 Angier.	M	14.3	13.5	9.3	14.4	14.1	9.5	15.1	12.7	11.1	12.6	13.7	11.0	16.6
R.F.E.O.	c	+3.3	+2.5	−0.7	+3.4	+4.1	−0.5	+5.1	+1.7	+1.1	+1.6	+3.7	0.0	+5.6
	v	1.7	1.9	2.0	2.4	2.0	1.1	2.4	2.8	2.0	2.0	2.7	1.3	3.5
	N	11	10	10	9	10	10.5	10	10	11	10	11	11	11
Angier.	M	14.4	4.8	2.5	3.6	5.8	6.7	8.0	11.0	13.5	11.1	13.9	10.1	11.0
R.F.E.C.	c	+3.4	−5.2	−7.5	−5.4	−4.2	−3.8	−2.0	+1.0	+2.5	+1.1	+2.9	−0.9	0.0
	v	3.2	2.7	2.3	3.6	2.7	2.3	1.3	2.8	2.8	1.4	2.4	1.8	2.2
	N	97	97	98	97	98	99	97	98	95	98	99	99	100
Huggins.	M	107.6	107.8	100.4	114.2	98.0	110.3	89.4	102.5	99.2	100.8	106.4	101.5	108.9
L.F.E.O.	c	+10.6	+10.8	+2.4	+17.2	0.0	+11.3	−7.6	+4.5	+4.2	+2.8	+7.4	+2.5	+8.9
	v	5.9	0.6	4.8	6.5	5.3	6.7	6.4	5.5	7.8	6.0	4.7	7.0	12.7

Huggins. L.F.E.C.	N	97	98	100	97	95	95	99	99	97	98	99	99	100
	M	102.8	104.3	115.1	101.4	94.1	102.0	106.4	93.4	87.7	98.7	108.4	97.7	110.8
	c	+5.8	+6.3	+15.1	+4.4	−0.9	+7.0	+7.4	−5.6	−9.3	+0.7	+9.4	−1.3	+10.8
	v	5.8	8.6	10.8	9.9	7.9	10.4	8.1	6.8	9.4	5.8	6.9	5.3	6.1
20 Lenfest. L.F.E.O.	N	11	11	10	10	10	10	10	10	11	10	11	11	10
	M	16.1	13.5	11.6	10.9	10.9	14.2	11.7	10.5	12.4	11.1	12.2	8.7	12.9
	c	+5.1	+2.5	+1.6	+0.9	+0.9	+4.2	+1.7	+0.5	+1.4	+1.1	+1.2	−2.3	+2.9
	v	2.8	3.0	3.0	3.0	1.2	1.9	1.0	1.6	3.2	5.1	1.9	1.7	4.2
Lenfest. L.F.E.C.	N	12	11	10	10	10	11	10	10	10	10	11	11	10
	M	20.4	16.8	11.6	11.2	11.8	14.0	9.0	9.3	6.2	10.0	8.5	5.1	8.3
	c	+8.4	+5.8	+1.6	+1.2	+1.8	+3.0	−1.0	−0.7	−3.8	0.0	−1.5	−5.9	−1.7
	v	2.5	3.6	3.0	1.9	1.6	2.0	3.0	1.2	1.9	0.3	1.8	1.7	0.8
George. L.F.E.O.	N	98	97	94	98	98	97	98	98	97	96	98	98	97
	M	104.0	100.4	102.6	99.3	105.1	111.1	101.9	100.5	96.4	102.0	97.4	94.2	95.8
	c	+6.0	+3.4	+8.6	+1.3	+7.1	+14.1	+3.9	+2.5	−0.6	+6.0	−0.6	−3.8	−1.2
	v	6.4	11.3	9.6	7.7	5.3	8.0	3.7	5.6	5.7	5.7	9.1	7.8	3.6
George. L.F.E.C.	N	98	97	94	97	97	98	98	97	96	98	98	99	100
	M	93.6	81.2	94.7	92.7	104.2	99.3	93.4	89.6	96.4	93.0	87.3	101.8	87.4
	c	−4.4	−15.8	+0.7	−4.3	+7.2	+1.3	−4.6	−7.4	+0.4	−5.0	−9.7	+2.8	−12.6
	v	9.1	8.9	5.5	4.6	5.8	6.9	4.9	6.6	7.8	4.8	7.7	5.6	5.7
Moore. L.F.E.O.	N	97	99	98	97	96	96	97	98	99	98	99	99	99
	M	106.1	106.9	105.2	103.0	104.7	108.6	102.7	106.9	110.9	105.7	111.2	99.9	93.0
	c	+9.1	+7.9	+7.2	+6.0	+8.7	+12.6	+5.7	+8.9	+11.9	+7.7	+12.2	+0.9	−6.0
	v	4.3	5.6	4.6	5.2	8.0	6.0	7.4	6.9	4.8	9.0	4.7	11.4	1.8
Moore. L.F.E.C.	N	96	97	97	97	96	97	96	97	99	98	97	98	99
	M	119.2	79.8	87.3	79.6	81.6	91.2	96.5	106.2	100.4	91.9	104.0	88.6	80.9
	c	+23.2	−17.2	−9.7	−17.4	−14.4	−5.8	+0.5	+9.2	+1.4	−6.1	+7.0	−9.4	−18.1
	v	16.1	6.7	11.6	7.2	7.5	4.1	4.0	6.6	4.0	6.1	5.4	6.6	8.2

23 Lenfest. L.F.E.O.	N	98	97	95	96	96	97	99	97	97	98	98	98	99
	M	105.7	110.8	97.4	93.8	97.8	100.2	99.9	92.9	86.1	96.6	91.9	80.9	83.0
	c	+7.7	+13.8	+2.4	−2.2	+1.8	+3.2	+0.9	−4.1	−10.9	−1.4	−6.1	−17.1	−16.0
	v	9.3	1.5	3.6	5.1	3.6	4.4	2.9	3.3	6.7	5.0	8.2	6.0	7.8
Lenfest. L.F.E.C.	N	96	94	96	96	96	94	96	97	97	97	100	100	100
	M	111.3	95.0	98.3	101.7	101.7	110.1	98.8	79.0	88.9	72.7	88.4	69.7	80.8
	c	+15.3	+1.0	+2.3	+5.7	+5.7	+16.1	+2.8	−80.0	−8.1	−24.3	−11.6	−30.3	−19.2
	v	7.1	5.1	5.8	2.7	5.4	7.8	12.1	3.7	5.6	8.0	7.4	8.4	7.0
Huggins. L.H.E.O.	N	96	97	96	96	98	98	97	97	95	98	98	99	97
	M	113.5	105.7	105.5	97.7	92.5	98.4	92.5	102.0	95.0	112.3	104.5	108.7	95.7
	c	+17.5	+8.7	+9.5	+1.7	−5.5	+0.4	−4.5	+5.0	0.0	+14.3	+6.5	+9.7	−1.3
	v	4.2	2.4	3.6	5.0	3.5	3.1	4.2	5.8	4.7	6.4	5.8	7.1	4.4
Huggins. L.H.E.C.	N	98	97	97	97	97	99	99	97	97	97	98	98	96
	M	103.5	82.8	86.1	87.8	87.7	94.2	92.1	94.5	99.9	111.5	101.3	113.7	95.1
	c	+55	−14.2	−10.9	−9.2	−9.3	−4.8	−6.9	−2.5	+2.9	+14.5	+3.3	+15.7	−0.9
	v	8.4	6.7	5.4	3.5	6.7	5.4	6.0	5.8	4.8	13.3	4.7	9.6	2.6
27 Lenfest. R.F.E.O.	N		96	95	96	96	96	97	96	96	99	102	101	100
	M		101.6	93.4	91.2	90.0	97.5	93.2	93.4	89.8	97.5	88.6	96.8	66.2
	c		+5.6	−1.6	−4.8	−6.0	+1.5	−3.8	−2.6	−6.2	−1.5	−13.4	−4.2	−33.8
	v		6.2	5.1	3.4	2.8	5.5	4.5	3.7	5.5	3.6	4.3	4.9	8.1
Lenfest. R.F.E.C.	N	96		96	97	97	97	98	98	95	98	100	99	100
	M	103.8		101.2	94.0	100.3	96.4	101.2	105.0	78.0	98.5	88.8	85.1	65.8
	c	+7.8		+5.2	−3.0	+3.3	−2.6	+3.2	+7.0	−17.0	+0.5	−11.2	−13.9	−34.2
	v	6.2		7.4	4.5	3.8	3.5	4.7	2.4	4.7	4.3	7.0	6.9	5.9

The records are averaged for nine subjects, three of them being left-handed. For the right hand we find, for mean error, a reduced error with visual control.

For constant errors, a similar result is apparent; when following the eyes-closed curve one may note a large negative error 20–50 beats, and a similar but larger positive error 70–160 beats, with a falling to a negative error again at 200 beats.

This may be interpreted to mean a groping for the correct length of line at the lower speeds when some time for reflective processes is allowed, and an inhibitory effect on the motor discharge; later the speed prevents this discrimination, and introspective testimony goes to show that a mental conception of a barrier, beyond which one cannot carry the pencil, is set up and kept more or less constant through the help of the joint and muscular sensations. It would follow, then, that this muscular stop is overestimated where reflection is not possible.

Finally, the falling-off of the length of the line is probably due to physical inability to rule a line of the full length of 140 mm. at 200 beats per minute and an examination of some individual cases confirms this opinion, for the lines may be started some distance away from the origin apparently in order to end them at the correct point.

Curve inclinations are upward, for mean errors, with visual control, while the eyes-closed records show no increase in error for the increased speeds.

For constant errors, with visual control, there is a similar inclination downward for both hands, with a 0 error at about 120 beats. It should be noted that this opposite tendency in mean and constant errors suggests that they should be kept separate in all computation.

The left-handed subjects have much better control of their left hand than have the right-handed subjects, and they may dispense with visual control to a large extent.

On the other hand, for right-hand records we find much the same increase in irregularity and error for both left- and right-handed subjects; they all must depend on visual control for reduction of errors.

It follows that the non-visual control exerted by the left-handed subjects on the right hand is as good or as great as for the right-handed subjects; while they have the hand in which they may be expected to excel under much better control.

It is not intended to present this as an argument for teaching left-handedness, but it is certainly suggestive when considering the question that ambidexterity be taught in early life.

It should be noted that two of the three left-handed subjects might be expected, because of special training, to show marked manual dexterity, while only one of the four right-handed subjects has had special training along this line.

No extended discussion is appropriate here as to the question of what portion of this extra ability of the left-handed subjects to react accurately is due to practice and habit, *i. e.*, is automatic, and accomplished without reference to the sensory motor by-path to the cerebral cortex; and on the other hand, as to whether the direct sensory motor path via spinal cord or medulla is not cut off entirely.

For 140 mm. averages and free motion, we find in general

(1) a reduced error and greater uniformity of result at all speeds where visual control is added, in the case of both mean and constant errors and for all subjects;

(2) the mean errors for visual-control records show a rise along a line whose equation is approximately $y = px$, or the equation of a straight line, where p is an undetermined constant.

On the other hand,

(3) the eyes-closed mean errors show no increase or decrease in value during the entire series;

(4) the constant errors for visual-control records show a drop from positive errors to negative errors, along a line whose equation is approximately $y = qx$ or the equation of a straight line, where q is an unknown constant, somewhat less in value than p in the case of mean errors; the constant error becomes 0 at about 120 beats;

(5) the eyes-closed constant errors follow the same equation for left-handed subjects, using the left hand, but all other cases suggest a curve of the parabolic form, having 0 constant errors at 60 and 180 beats and being convex upward.

Considering individual records for 14 cm.

A general survey of the charts suggests certain irregularities that call for explanation, for there will be sudden large increases in errors, that are explicable on the hypothesis that the subject has temporarily lost control of the moving hand, that is, that fatigue is to be noted.

While the purpose of the investigation has been to allow no lines to be ruled while the subject was conscious of any such feeling, there being a pause of any desired length to permit time for rest, it is to be noted that a considerable amount of recorded data as to fatigue shows that it is an unconscious or subconscious phenomena.

Further, the series of records have been arranged to occur from 20 to 200 beats and never in the reverse order, because of subjective

limitations, so it is reasonable to expect that during the period of twenty minutes to one half an hour required for a series of records, there will be lapses of volitional control entirely beyond the ken of the subjects. It is to this cause rather than to pure chance that the results will be attributed. With this exception, the individual records show close agreement with their average.

The results obtained from a consideration of free hand-movements of 1, 10, and 14 cm. length are:

For 14 cm. lines

for the average of nine subjects:

The mean errors,

(1) increase with speed for eyes open;

(2) do not change in error with speed-change for eyes closed;

(3) visual control reduces errors for right hand, but does not for the left;

(4) right-handed subjects alone gain from visual control.

The constant errors,

(1) decrease with speed in visual cases;

(2) increase with speed to the middle and then reduce to 200 beats for eyes closed;

(3) left-handed subjects are more accurate for the left hand and can dispense with visual control;

(4) all subjects need visual control for the right hand;

(5) left-handed subjects show less error throughout for non-visual.

For the individual cases,

the mean errors

(1) show evidences of loss of control or fatigue for some speeds, and the average results are confirmed.

The constant errors show that the average deductions are confirmed.

Lines 10 cm. long:

Averages for seven subjects as regards mean errors have especial interest for l.h.e.c. records, which alone show a rise of error with speed-increase.

Noting that the records are overwhelmingly averages of right-handed subjects (six to one), it is of interest to examine this record.

We may say, then, that for right-handed subjects, the voluntary control for the right hand is not much improved by the introduction of visual assistance; it is more marked for speeds of 100 beats or less than for the high speeds. And in the latter case, it is under 10 %; but when the left hand is considered, a marked gain or 40 or 50 %

is apparent, when the eyes are used except for the two lowest speeds.

As far as it is possible to offer any hypothesis from the few facts tabulated, it may be said that right-handedness implies a high development of muscular control, but slightly improved by the introduction of the visual element, as far as the right hand is concerned; but for the left hand muscular control comparable to the right-hand control can be obtained only with visual control; in short, I fail to find evidences of cross-education, where the visual element is absent, nearer than about 50 % of the mean error.

No clearly marked gain through visual control can be pointed to in the case of constant errors; there is, to be sure, a slight gain in steadiness and error-reduction, where eyes help in the case of both hands, but not 5 % in magnitude of the difference noted with mean errors.

Individual records show fatigue-points at 40 to 80 beats and again above 140 beats, but there is no perceptible loss of control during a series of lines ruled at only one speed.

It is apparent, when comparing with the 140 mm. records, that there is no physiological reason why the subjects may not rule the full length of a 10 cm. line at 200 beats, and the limit of movement for high speeds is probably between 10 and 14 cm.

The constant errors are in general positive and only Me. shows a tendency to underrule lines at high speeds.

For 10 cm. lines,

for the average record,

(1) the mean-error curve is horizontal for l.h.e.o., but otherwise rises with speed-increase;

(2) visual control is a 10 % gain for right hand and 40 to 50 % for the left, so right-handedness is prominent for the eyes-closed series;

(3) the constant-error curve for r.h.e.c. rises, but all others show reduction of error as rate increases;

(4) visual-control gains are not over 5 %.

The individual curves show,

for mean errors,

(1) marked-fatigue points for l.h.e.o;

(2) r.h.e.o. curve is horizontal, but all others rise;

(3) l.h.e.o. curve is about the same as r.h.e.o., but there is more loss for non-visual series with left hand.

For constant errors,

(1) r.h.e.c. curve rises, but all others are horizontal;

(2) the eyes reduce errors especially for the left hand;

(3) overruling is prominent even at high speeds, for there is no evidence that the lines are shortened at high speeds.

Lines 1 cm. long:

Averages are made for nine subjects, three being left-handed.

The eyes are effective in reducing mean errors and to a less extent for constant errors.

A noteworthy feature of the constant-error record is that the errors are positive with one exception, that of 20 beats with l.h.e.c. and even this curve jumps rapidly above the o line.

In all cases the motor discharge is of sufficient magnitude to cause overruling in cases of normal lines of one cm. The assistance afforded by the eyes is not marked.

Certain evidence of an introspective character, that most of the subjects offer, is to the effect that, "when I would do good, evil is present with me"; that, where there is a decided feeling that the muscular limit, if such a term be permitted, is exceeded, yet the subject's will-power is not sufficient to inhibit the overruling; there is a more or less vivid conscious error in the 10 mm. series for the hands.

In regard to the relation of mean and constant errors, there is more close uniformity than with the 140 mm. lines, but it is to be noted that there is no comparison to be drawn between maximum or minimum points; for example, at 100 beats the minimum points for r.h.e.o. agree closely, but the maximum constant error matches the minimum mean error at 100 beats for r.h.e.c.

We cannot predict, then, that a subject capable of closely ruling to the normal will be able also to rule each line of the same length as the rest of the series, or *vice versa.*

As in the case of mean errors in general note that subjects show a less constant error and more regularity for their more dexterous member; it is not true for the left hand that for left-handed subjects visual control is a hindrance for accurate work; otherwise the same gain, by use of the eyes, is to be noted for the rest of the records.

Individual records show close correspondence with the average of results, and the latter may be considered fairly representative.

Almost the whole series shows the constant error positive, the most consistent example being for J. with l.h.e.o.; this tendency to overrun the 1 cm. lines is consistently uniform and has been elsewhere commented on, so it may be left with the observation that the log shows that the subjects were frequently conscious of this overruling, but confessed inability to correct it.

Only in the case of Y. for the three left-handed subjects and for W. among the six right-handed men does the left hand show less mean error than the right hand, and all other cases show such an interweaving of curves as to render it difficult to perceive any advantage that the more dexterous hand possesses on the score of accuracy.

For constant errors:

For individual cases it is to be noted that for the left-handed subjects J. is better for the right hand, Le. is indifferent, and Y. prefers the left hand; while three of the six right-handed subjects prefer the left hand and one is indifferent; thus giving still further proof that a more dexterous hand is a fiction on the score of the right or left-handed theory, when accuracy of straight-line movement is to be considered.

For 1 cm. lines,

for the average, note

for the mean errors:

(1) visual control reduces errors;

(2) errors increase for eyes open, but decrease for eyes closed, as speed increases when considering right hand, but left-hand errors are constant;

(3) as left-handed subjects are better for r.h.e.o. than right-handed subjects, but not for eyes closed, it is suggested that visual control equalizes differences in the subject's less trained hand.

For constant errors:

(1) visual control reduces errors;

(2) curves are horizontal in all cases;

(3) all errors are positive, showing consistent overruling;

(4) as visual control of the left hand is a gain for right-handed but a hindrance for the left-handed subjects, the more practised hand is probably able to dispense with visual control, and depend largely on the muscular sense.

Mean and constant errors are not comparable. For the individual cases we find a corroboration of the above and for mean errors: more dexterous hand does not excel, and evidence against ambidexterity is conflicting; for constant errors: overruling is consciously done.

Constrained hand-movements for lines 14 and 1 cm. long and for the weight, both accelerating and retarding the movement, are to be next considered.

Constrained motions are of two general types as examined by the writer. Series of the records for the hands were taken at 140 mm. and 10 mm. bases with a weight hung on the finger or fingers of the

hand under investigation; in one series the weight acted as a pull or accelerating effect on the ruled line and in the other series the weight was imposed as a retarding effect, tending to restrain the movement of the hand.

This weight was in all cases 260 grams, this weight being chosen as of sufficient amount to have a perceptible effect, but not large enough to cause feelings of pain or fatigue in any case.

The average for seven subjects, three being left-handed, is as follows:

In general, the mean errors for the right hand are less, and less variable as compared with the left hand. The left-hand records are very close to the corresponding right-hand curves, especially the portions of the eyes-closed records 20 to 120 beats.

This may be said for both mean and constant errors. In general, mean errors are reduced, and curves are more nearly straight lines when the weight is added; also the weight reduces constant errors, and gains more regular records at all speeds. It is to be noted as a point of unusual interest that there is no apparent shortening of the line ruled when the weight is hung on the hand, for the negative errors are less, not more when the weight is applied.

In general, then, the imposition of a weight that will be small enough not to cause pain or fatigue shows that both mean and constant errors are reduced; that the amount of error is less variable over the range of speeds used; that the records show no retarding effect, but that the subject is both able to move the hand just as far as without the weight, and do it with much greater accuracy.

Individual records for 14 cm. and weight-retarding show a marked reduction in both mean and constant errors, and a less marked gain in uniformity in every case. This tends to confirm the introspective opinion of W. subject that the imposition of a retarding weight tends to reduce errors of both classes and to cause greater steadiness.

It should be added that there is evidence of an occasional letting-go of voluntary control, so to speak, resulting in a large increase in mean error, as already pointed out, or a large increase in negative constant error, as shown on all individual records, and it would seem then that the matter of cortical control is more vital and indispensable for the restricted movements.

The effect of weight-retardation on visual records is to reduce the error and steady the ruling of the less dexterous hand to a much more marked degree than for the well-trained hand.

In the l.h.e.c. records the lack of corrective effect of visual control

is marked, as in the case of free movements, but the dip in the curve at 30 to 70 is not noted in the free ruling and should be considered as a distinct shortening due to weight-retardation before discriminative processes have oriented the subject.

Without considering the accelerating weight-records in detail note that:

The effect of weights (less than that necessary to cause pain or fatigue), either tending to accelerate or retard motions, is to reduce both mean and constant errors and to render more uniform or more uniformly increasing or decreasing such errors, except in the case of l.h.e.c., where constant errors are greater positively with the weight-pulling and greater negatively with the weight-retarding, than for free motions; that is, the effect of the weight is natural, and shows no signs of inhibition in this particular case.

There is no such marked fluctuation in error for the pull-records as was noted for the weighted curves, and it is further noted that the individual pull-records are more bunched or consolidated about some mean than are the free-movement curves. This suggests that the accelerating weight is a decided help for accuracy and regularity, and it would seem to call for less voluntary control than for either of the other movements.

Further, as the effect of pulling weights is to equalize the accuracy of movement of the hands, the hypothesis is proposed that weights either accelerating or retarding the movements of the hand tend to equalize their accuracy or to promote ambidexterity as far as accuracy of straight-line mean errors is concerned.

L.h.e.c. rise for Ha., are horizontal for J., W., and Y., and slope downward for the other subjects, the net effect being a slight downward slope. The loss of accuracy and regularity when the visual sense is inhibited is to be noted in every case, it being especially marked for Bo., Li., and W.

As compared with the r.h.e.c., there is not sufficient evidence to lead to the conclusion that the right hand is a more accurate member than the left, but on the contrary the left-hand record for non-visual control is lower for both weighted series than is the right-hand curve. Contrasting this with the eyes-open records for free and weighted movements, the visually aided results show a greater accuracy and regularity for the right hand.

This leads to a proposition that the greater dexterity on the line of accuracy, of one hand, that is the right hand for right-handed subjects, and the left hand for left-handed subjects, is a matter of visual

control and is in no sense due to the muscular sense or to automatic action, for without eyes we are ambidextrous as far as accuracy of linear movements is concerned; the proposition needs careful scrutiny in application to the general question, but is held to be correct within the range of experiments.

We are tempted to extend this matter somewhat in the following way, by saying that there is no evidence deducible from this research that there is hereditary preponderance of activity or accuracy of one hand or one leg (as shown later) over its mate, and the baby is brought into the world with an equal capacity of accuracy of both members.

It is, then, an evolutionary matter, not racial but individualistic, and right-handedness or left-handedness is largely a development after birth. Our system of education is responsible for the over-development of one hand, and such a case as that of Dr. Anderson of the Yale University Gymnasium, who in class demonstration cannot instantly tell which hand is being used to actuate the chalk at the blackboard, is the normal symmetrically developed man.

The school reform for ambidextrous training is radical enough, but seems a logical conclusion of the argument. Apologies are appended for driving the argument beyond the limits of the investigation, but it is hoped that the enquiry is at least suggestive.

For 14 cm. lines,

weight-retarding movements:

For the average of nine subjects:

The weight reduces errors and promotes regularity in the case of both mean and constant errors, nor does it tend to cause underruling, save in the case of left-handed subjects for l.h.e.c. records. There is a gain, in general, when the visual factor is introduced.

For mean errors,

(1) right-hand curves are horizontal, while the visual records show increasing error and l.h.e.c. a reduction of errors;

(2) the right hand gives slightly better results;

(3) note that l.h.e.c. record is equally good for free or weighted movements.

For constant errors,

(1) r.h.e.o. and l.h.e.o. curve downward, while both non-visual curves slope upward;

(2) the left hand seems equally efficient, as compared with the right hand.

For individual cases,

note (1) fatigue-spots are more numerous than for free movements, especially for the left hand;

(2) weight reduces both mean and constant errors and to a less extent even records.

For mean errors,

(1) visual control reduces errors;

(2) the weight tends to equalize the accuracy of the right and left hands.

For constant errors,

(1) there is no general testimony showing shortening of lines at high speeds;

(2) the less trained hand is more helped by the weight, especially for non-visual work.

The evidence for right- and left-handed subjects is inconclusive, and we cannot finally say that the more trained hand is capable of greater accuracy.

Weight-accelerating movements:

The average of seven subjects:

The accelerating weight reduces mean and constant errors, and improves regularity of curves, except for l.h.e.c. constant-error record. There is some evidence that a pull causes overruling, while a retarding weight causes underruling, but there are exceptions enough to warrant care in finally accepting this statement. Visual control with accelerating weight reduces error more than the weight acting alone.

For mean errors,

(1) weight reduces errors for r.h.e.o. and l.h.e.c. as compared with free-movement records, while the other two curves are inconclusive;

(2) visual sense helps in accurate ruling;

(3) non-visual records are not reduced, as a rule, from the results of free motion.

For constant errors,

(1) the accelerating weight tends to greater accuracy, with an exception for the non-visual records.

No testimony of marked importance is to be noted in comparison of right-handed and left-handed subjects; the more trained hand shows greater accuracy in some cases, but fails to excel in others; so the data is inconclusive.

For individual cases we find:

(1) the acceleration records are more accurate and regular, and present fewer lapses than the free or retardation results, suggesting greater ease with weight assisting;

(2) visual control is prominent throughout, and evidence shows that this sense is the greatest factor in the predominance of the more trained hand; the non-visual records should and do show no marked difference in the hands;

(3) a weight tends to equalize accuracy of hands;

(4) the overruling effect of weight is over-corrected in some cases for constant error of low rates.

Constrained movements of 1 cm.:

The average is of seven subjects, three of them being left-handed:

With weight-retarding movement, there is no reduction of mean error with visual control of right hand, but there is with the left. Constant errors show little reduction for either hand with eyes open.

The facts would seem to warrant the hypothesis that, for the left hand, a movement uncontrolled visually, whether restricted by a weight or not, can be made with greater accuracy, when time is permitted for discriminative and reflective processes and visual-control results in about the same error whatever the speed, while the right-hand motions show no such evening effect of visual control with the weight-records or even reduction of error; the free movement, however, does show a reduction of error.

A general statement may be deduced that, for lines of 10 mm. in length, there is no difference in either mean or constant errors, when a weight is imposed to cause retardation, provided the weight is not large enough to cause pain or fatigue.

By separating the averages for right- and left-handed subjects, it may be further said that:

Visual control is not efficient to reduce the error and no particular gain in regularity can be noted. The left-handed subjects show, for the left hand, much better results without visual control as far as the free motion is concerned.

While somewhat contradictory, it may be stated that constant errors are reduced by the weight addition, and there is some evidence leading to the belief that the ruled line is shorter when the weight acts as a retarding influence.

Individual Records

Considering lines 10 mm. long with a retarding weight:

A glance over the seven individual records shows some considerable increase in both constant- and mean-error irregularities, as compared with the free-motion curves, as well as in actual errors; there

are distinct losses of volitional control for both classes of errors, especially at or near the ends of the series.

There are cases of very low mean error to be found on all records, where the value is ½ mm. or less, and, while the same phenomenon is found with free motion, it is more marked here and occurs more frequently; in most cases it seems as a drop from errors of larger values rather than a gradual matter, as if the subject realized the large error and exerted unusual volitional control to correct and produce a very accurate record, but found that the attention needed was beyond his will-power, as shown by the immediate lapse of accuracy.

There is an indirect confirmation of this view from the introspective testimony of the subjects.

The visual element steadies but does not reduce mean errors when weight is retarding.

The general shape of curve for eyes closed is downward 20–40 beats, and rising for the rest of the series; it is less regular, but more accurate than the visual results.

The fact that constant errors are mostly positive leads to a denial of any inhibitory effect of the retarding weight.

For 1 cm. lines,

weight-retarding movements:

For the average of seven subjects we find:

(1) visual control does not improve accuracy or regularity as in free movements;

(2) a retarding weight tends to make errors constant whatever the speed-rate;

(3) the testimony goes to show that the free-movement records are more accurate than the retardation ones.

Mean errors are:

(1) no more accurate and perhaps less regular, when the weight is imposed;

(2) right- and left-handed subjects are equally accurate.

Constant errors:

(1) the more dexterous hand is superior for coördinations requiring accuracy;

(2) ruled lines are slightly shortened in some cases;

(3) weight-records do not give more accurate results as compared with free movements.

For individual records we find:

(1) retarding weights increase errors and irregularity;

(2) fatigue-points are more marked and frequent than for free movements.

Mean errors,

(1) the visual factor is of some value, but the testimony is varied; right hand for increased regularity only, and left hand for greater accuracy only;

(2) curves are horizontal or reducing with speed-increase.

Constant errors,

(1) the more dexterous hand coördinates better;

(2) all errors are positive;

(3) visual control helps only for regularity;

(4) curves are horizontal or rising.

With weight-accelerating movements, the average record shows a sudden rise in mean error at both ends, not in evidence with free or retardation results.

In general it is to be noted:

(1) that the visual element is of no value for reducing the error, and of little value for promoting regularity;

(2) that the pull-records are closely comparable to the free-motion records, and the accelerating influence of the weight is imperceptible;

(3) that the pull-records are more regular and closer to the free-motion curves than are the weighted records, especially at the ends of the left-hand curves.

For constant errors:

It is more in accord with the facts to say that the imposition of a weight tends to reduce the constant error, and this is more marked when the weight acts in pulling or to accelerate the motion.

Comparing with the weighted curve, we find the same general type of rising curve, similarly located, and the same is true when compared with the free-motion curve. Constant errors are reduced, but slightly, and visual control is rendered nil, when the weight acts either to accelerate or retard the movement, and of the two, the accelerating effect is more marked, as reducing errors and promoting regularity.

There is no appreciable tendency for the weight to reduce the ruled lines when retarding motion, nor is the weight as accelerating, able to extend the line beyond the point set in the free motion.

When contrasting averages from right- and left-handed subjects it may be said:

As compared with free motions there is a slight reduction of error

and irregularity more marked with the left-handed subjects, but a general close correspondence of results.

The question is now appropriate, why should the right-handed men show a reduced error for speed-increase, while the left-handed subjects show the reverse? Bearing in mind that the right hand is the more dexterous or better trained in the former case, it may be suggested that the order of record from 20 toward 200 beats is such as to cause more accurate results at the upper limit, in spite of the fact that less time is allowed for discrimination and adjustments; on the other hand, left-handed subjects have much less advantage of practice and habit in their use of the right hand, and will show the predominance of error, when the ruling is too rapid for careful discrimination.

It becomes a struggle between automatism, or semi-automatism, on the one hand, and discriminative processes on the other.

Visual control is not an advantage in the case of accelerating weight, and the large reduction in error with visual control for the free movements is not evident with weighted motions.

For the left-handed subjects we find that the eyes-closed record shows closer work than does the eyes-open curve; it is lower and nearer the line of o error; in this respect, it shows the same effect as with the free-motion curve, and to a less extent as for the retardation weight-record. The accelerating record is, however, more accurate and regular than either of the other curves.

It will be clear, then, as observed, that for constant errors, visual control tends to reduce errors and steady records whatever the speed-increase, as far as right-handed subjects are concerned, but this effect is not noted for left-handed subjects using the right hand, and, with their left hand, visual control is a disturbing element.

Further this erratic effect of visual control is less marked but clear when the weight acts as a retarding factor, but is much more noticeable for the free-motion record.

Individual records show few lapses of control for either errors.

The bulk of the evidence is that the weight imposition, whether acting as a retarding or accelerating influence, is effective in rendering the results more accurate and regular, though at least one subject exhibits the opposite effect for the accelerating weight.

The left hand is better for J., Le., and W., but is less regular for all subjects, save Le. and W., showing again a somewhat complex mass of testimony, from which we may conclude that the right hand is the more accurate member for right-handed subjects, and to a

much less extent the left hand is preferred by the left-handed subjects.

Visual control is to be noted as effective for accuracy and regularity, except for Ha., where the curves closely intertwine, and for J., where the eyes-closed record is much better.

Weight-accelerating movements:

For the average of seven subjects we find

visual control is of doubtful advantage, for left-handed subjects, but shows a clearly marked reduction of error for right-handed subjects.

Mean errors are:

(1) similar in all respects to free-movement results;

(2) acceleration-curves are closer to free-movement results than are retardation records;

(3) the more trained hand shows reducing error for speed-increase, while the other hand shows increasing errors, because of superiority of practice-effects over the native tendency to increase error as speed-rate rises, for the more dexterous hand alone.

Constant errors:

(1) there is no tendency to overrule, as compared with free movements, when weight acts to accelerate movements, for there are even cases of lines being shortened with accelerating weights;

(2) a weight seems to negate the results of visual control, as a rule.

For individual records we find:

(1) fatigue-points, for the right hand only, are to be found in a few cases;

(2) weight promotes regularity and accuracy;

(3) visual control is effective only for reducing variations of error;

(4) the better trained hand is the more accurate in the records, to a slight extent;

(5) there are evidences of semi-hypnotic or dreamy states in the non-visual series.

Compound Motions

Series of records were taken at 100 mm. and 10 mm. bases for the hands, with what is called compound motion. This consisted in an additional movement of the hand that was not ruling with the pencil, in a similar manner, as regards the amplitude and general character of the motion, but in an opposite direction.

For example, suppose the left hand is ruling a 100 mm. line out-

ward, or to the left; coincident with this movement would be a similar motion of the right hand outward or to the right. The origin of both motions, or the starting-ends of actual and imaginary ruled lines, was optional, it being desired to bring out the effect of such additional motion, as little complicated as possible with restrictions, as to its position or extent. Actually this distance varied from about 10 mm. where both motions were outward to 600 mm. for inward motions.

A comparison of such compound motions with single-hand records shows in general the following:

For 10 cm. lines:

The case for mean errors may be summed up by saying:

(1) left-hand records are less accurate and regular than the right-hand curves;

(2) visual control reduces error and irregularity in all cases, but is more marked with the left hand;

(3) errors increase with speed-increase;

(4) compound-motion records show little increase in error or irregularity, as compared with the simple motions.

For constant errors:

No marked peculiarities are to be noted, but in general,

(1) left-hand records are less accurate and regular;

(2) visual control reduces errors and irregularity;

(3) errors reduce with increase of speed, except for compound motion uncontrolled visually;

(4) compound-motion errors are not much greater, nor is the irregularity increased.

Individual Cases

100 mm. hand with compound motions.

A glance at the charts shows for individual records a few examples of inhibition of voluntary control for both constant and mean errors, it being much more marked in the case of mean errors.

These lapses of control appear for constant errors for A. with l.h.e.c. at 160 and 180 beats, for mean errors for G. with l.h.e.c. at 200 and with r.h.e.c. at 180 beats; for Le. with l.h.e.c. at 50 and with r.h.e.c. at 160 beats; for A. with l.h.e.c. at 200 beats; thus giving evidence that the visual element has a steadying effect, and that the left hand is less reliable save for Le.

There seems reason for contending that the compound motion

can be carried out, as arranged, without loss of accuracy or regularity on the part of the ruling hand, and further that the subjects are pretty generally apt to react to a given stimulus within certain rather narrow limits of accuracy.

The evidence is here pretty conclusive that the right-handed subjects, as a whole, show greater accuracy by about 25 % for the more dexterous hand; but it will be wise to consider the individual cases on this point.

Greater regularity and accuracy for the right hand is attained by all right-handed subjects, while the preference of Le. for the left hand is clear but much less definite.

For individual cases:

The evidence again is fairly well marked that the more practised hand will give a better account of itself even when visual control is not called on.

The results for compound movements of the hand for 1 and 10 cm. lines are summarized as follows:

It should be kept in mind that the compound records were in all cases taken in connection with a duplicate series of lines for one hand, and called simple movements. These simple movements correspond with the free-movement records that have been considered already.

The purpose has been to bring out the modification of results that a compound movement introduces, rather than to bear heavily on intrinsic phenomena, *i. e.*, comparison is deemed more important.

For lines 10 cm. long:

Average of seven subjects:

We find for mean and constant errors:

(1) left-hand records are less accurate and uniform;

(2) visual control increases accuracy and regularity, especially for the left hand;

(3) there is an increase as the speed increases for mean errors, and a decrease for constant errors;

(4) compound movements are practically as accurate and regular as the simple ones for constant errors.

For individual cases:

(1) a few lapses of control or fatigue-spots more marked for mean errors with the l.h.e.c., for the visual sense steadies ruling, and the left hand is less reliable;

(2) compound and simple records show close agreement;

(3) the more trained hand reacts more accurately, and with greater regularity;

(4) non-visual records show a cautionary shortening of line at low speeds, and another at the upper limit, the latter being due to physiological limitations.

For 1 cm. lines:

As far as averages are considered:

We may say, then, for mean errors:

(1) that visual control is effective for reducing errors, and increasing steadiness in both sets of records, being more marked with the less trained hand, the left;

(2) only in the case of the l.h.e.c. curves is the movement of the free hand noted as appreciably affecting the accuracy or steadiness of the record.

(3) eyes-closed records in general show a considerably greater error at 20 beats that practice rapidly reduces up to 40 to 60 beats.

We may note for constant errors:

(1) all errors are positive and confirm the earlier deductions on this point;

(2) visual control reduces error and improves steadiness of record;

(3) the free hand-movement does not affect either the accuracy or uniformity of results;

(4) errors do not increase with speed.

For individual records:

Comparing the individual cases of simple and compound movement, there is no particular reason for concluding that the compound movement is a disturbing influence as far as the records of all subjects are concerned, save possibly the lapse of G. at 20 beats, and on the other hand a case of greater accuracy and evenness for compound movements for Mo. with r.h.e.c. constant error.

It is, then, possible to extend the conclusion of the 10 cm. records, and say that both lengths of lines are ruled with a fairly constant limit of error, whether the movement be simple or complicated by movement of the free hand.

Individually there is testimony in favor of the gain in accuracy with visual control for Hu., Hy., and Le., while the crossing of curves for the other subjects shows that there is no difference in eyes-open and eyes-closed results, the general conclusion being in favor of the value of the eyes for accurate results.

For mean errors the right hand is more efficient in the case of A., Hu., Hy., and Me., while the reverse is the case for the rest, and the evidence goes to suggest that greater accuracy can be attained with the more practised hand.

For constant errors the right hand is more accurate in the case of A., G., Hy., only; Me. and Mo. are equally accurate with the hands, and the rest show a marked preference for the left hand, the evidence being thus conflicting, pointing to the theory of ambidextrous development on the lines of accuracy.

L.h.e.c. records are horizontal for all subjects except Hu., Me., and Mo., who show an upward slope to the curve. Evidences of visual control as giving greater accuracy are noted in general above 70 beats and individually for Hy. and Mo., only, the remaining records being so intertwined that no difference can be noted, all suggesting that the eyes are of but little assistance when the left hand is considered. The right hand is preferred with eyes closed by A., Hu., and Le., while four right-handed subjects testify that the less trained hand is more accurate.

The testimony here seems conclusive as pointing to a denial of the current notion as to the greater accuracy of the right hand for right-handed subjects, and of the left hand for left-handed subjects, and further suggests that visual control is a large factor in the supposed superior excellence of the hand mentioned.

Summarizing

For lines 1 cm. long:

Average of seven subjects:

It may be said that:

(1) visual control reduces both mean and constant errors, especially for left hand;

(2) errors are constant whatever the speed;

(3) constant errors are positive showing overruling in all cases;

(4) there is no disturbance created by the second-hand movement, save for r.h.e.c. mean errors, where the accuracy is less for the compound records; this is probably due to the fact that this record shows the least evidence of voluntary control, and is thus most subject to disturbances;

(5) there is a marked reduction of mean error 20–50 beats, probably due to practice.

For individual cases note:

(1) fatigue-spots for non-visual mean errors only;

(2) the equality of result of both types of movements is noted for all cases;

(3) the non-visual right-hand records for some subjects are more accurate;

(4) the more trained hand is not, as a rule and subject to exceptions, the more accurate one, especially for the non-visual records; and

(5) there is evidence that the superior accuracy of the right hand for right-handed subjects is largely a matter of visual control.

Head-Records

There was no attempt made to differentiate the visual element because the very movements of the head prevent the full use of the eyes; as a matter of fact, the subject's attempt to make use of the eyes and the aid is more marked at slow speeds and upon facing the apparatus. It is to be noted here that the visual element, as reducing the error at low speeds, is equally marked whether the eyes are directed toward the recording pencil or not. This raises an interesting question as to the direction the eyes must take for the optimal result; must the eyes be fixed on the moving pencil, on its immediate surroundings, or may they wander at will about the surrounding objects?

My own introspective testimony, corroborated by others, who have acted as subjects for this investigation, is that the eyes are most effective when gathering spatial relations in a gross way, and it may be expected that the effects of visual control as reducing errors will be equally efficient, whether the recording pencil be screened or visible, provided it be possible to bring on the retina objects that are grouped about the centre of attraction, the pencil, but not in its immediate neighborhood.

The records show that there is underruling at the higher speeds because of physiological limitations; but this shortening is greater for the backward movements, for the position of the subject is such as to lead to greater uncertainty as to the exact length of ruled line, and it is probable that a cautionary or inhibitory feeling is the cause of this shortening beyond what will be clearly due to inability to perform the desired movement.

Further, visual control is effective, in the case of constant errors, in lengthening the ruled lines at high speeds, and thus reducing the negative constant error.

While the muscular control of the head is a constant, whether the movement be forward or backward, it is less effective for constant error reduction when the head is moved backward. Consequently, while the backward and forward curves are fairly well in correspondence, there is some reason for offering the proposition

that either the eyes are of assistance in forward movements to reduce mean errors at high speeds, and they are of no such value for backward movements, or the muscular control of the platysma myoides, trapezius and associated muscles of the neck group is more nearly perfect for movements of the head forward than for backward motions, the latter being to my mind the better hypothesis.

The results for head-movements for lines 1 and 10 cm. long are summarized:

For lines of 10 cm. length:

Average of six subjects:

For mean errors:

(1) the curve for head-forward and head-backward closely corresponds to l.h.e.c. record; the errors increase by 50 % with increase of speed-rate, suggesting that

(*a*) visual control is negligible, as far as seeing the moving pencil is concerned;

(*b*) control of head for forward equals that for backward movements.

For constant errors:

(1) there is underruling at high speeds because of the usual physiological limitations, and this is more marked for head backward results, suggesting that

(*a*) spatial relations are obtained, when the apparatus is visible, that tend to correct underruling, or

(*b*) an extra inhibitory effect, due to lack of knowledge of spatial relations, is added to the normal physical shortening and the subject moves the head a less distance than is naturally possible; or

(*c*) the muscular control is less complete for movements of the head backward.

For individual cases we find:

(1) fatigue-lapses are less in magnitude than for the hands, because the head-movement can be only a fraction of the forearm-movement;

(2) mean errors increase and constant errors decrease with speed-rise;

(3) similarity of individual head-forward and head-backward curves is suggestive, taken with the fact that no typical form of curve is to be found;

(4) head-backward constant errors are greater and less regular in all cases, suggesting that the eyes, in head-forward records, by getting spatial relations, are more efficient.

For lines 1 cm. long:

Average of six subjects:

For mean errors note:

(1) the head-backward records are less regular than the head-forward ones, and rise a little with speed-increase, showing visual assistance for accuracy or better muscular control for the forward movements or both;

(2) the constant errors show shortening of ruled lines at high speeds a little more marked for the head-forward results;

(3) there is constant overruling.

Individual cases suggest:

(1) fatigue-spots are apparent, especially for head-backward movements;

(2) errors do not increase with speed;

(3) the movements of the head forward are under better control.

Foot-Records

10 cm. records show that

(1) the eyes are of no assistance as to increasing accuracy but help in promoting regularity of error;

(2) a shortening of ruled lines with speed-increase is noticeable, and is probably due to the usual physiological reason;

(3) the feet are capable of less accurate motion than the hands, but show better results than the head;

(4) mean errors increase but constant errors decrease with speed-increase.

Individual records show:

(1) less violent fluctuations of errors in all respects than do the results of head or hands, for vertical foot-movements are of less extreme extent than are arm- or head-motions;

(2) that for visual control with mean errors, no foot is the more accurate, and there is no reason to believe that the feet are unequally educated.

1 cm. records show, as far as mean errors are concerned, that:

(1) visual control is of no value as either reducing actual errors or as effecting greater regularity;

(2) Errors for foot-movements are no less, but considerably more regular than for head-motions;

(3) errors for hand-movements are more regular, and only 50 % of the results for either head- or foot-movements;

(4) all curves are horizontal;

(5) there is no appreciable advantage as to accuracy or regularity that can be attributed to either foot. The evidence goes to show that the subjects are ambipedalous, if it be permitted to coin such a word.

In general, we find that, as far as constant errors are concerned,

(1) visual control does not help to reduce actual errors or promote uniformity;

(2) errors for foot-movements are less than the head records, and but little greater than the hand results, while the regularity for the feet is comparable to the hand, and much greater than for the head;

(3) all curves are horizontal;

(4) there is no particular advantage that either foot has over the other either as to accuracy or regularity.

The evidence is that the subjects were ambipedalous, as far as ability to reach a certain point equally well by either foot is concerned. The popular notion has been to the contrary, and it is a point of considerable importance to note the last point.

For example, in kicking, as developed by football trainers, it is commonly assumed that the right foot for right-handed subjects should be developed, and the opposite foot for left-handed men. Or again, in the case of a person lost in the woods and walking in a circle, it is observed that right-handed persons will turn to the left; probably because of the pace of the right foot being slightly longer than the left. My reply to this evidence will be that the data herein presented is for vertical movements of the foot, starting from the floor in every case, the subject being seated in a chair.

On the other hand, it is an entirely different movement, calling for a much different and greater muscular control in the case of kicking or walking that must be considered. For this reason the evidence, while conclusive within its range, is not offered as more than suggesting that the feet are equally well trained for the usual adjustments, and only an exhaustive investigation covering all possible foot-movements will settle the question.

The result for foot-movements for lines 1 and 10 cm. long is here summarized.

For lines 10 cm. in length:

Average of seven subjects:

Note in general that

(1) l.f.e.c. mean error is most erratic, while the same curve is the most accurate, as far as constant errors are concerned;

(2) the left foot mean and constant errors are slightly greater than those for the right foot, for visual records;

(3) mean errors increase and constant errors reduce with speed-increase;

(4) the visual sense improves regularity, but does not reduce errors;

(5) there is a physiological reason for the shortening of lines at high speeds;

(6) the feet are more under control than the head, but less than the hands.

For individual records:

(1) fatigue-lapses, all for non-visual, are less numerous and of less magnitude than for the hands and head, for the vertical movement of foot is likely to be of less extent than that of head and hands for the particular motion required here;

(2) there is no foot capable of being called more accurate than its mate;

(3) the eyes appear to be of no value for reducing or regulating errors for foot-movements.

For lines 1 cm. long:

Average of six subjects:

It may be said in general that

(1) the visual sense is valueless for promoting accuracy or regularity of curve;

(2) errors of foot-movements are more regular and, for the constant errors, more accurate than for the head-records;

(3) errors of foot-movements are less and less regular by 50 % as compared with records for the hands;

(4) errors do not increase or decrease with speed-changes;

(5) the feet are equally accurate.

For individual results:

(1) fatigue-lapses and cases of large error-increases are noted in a number of subjects, both for visual and non-visual records;

(2) further, evidence is available as to the indifference to visual control;

(3) no preference for either foot is to be discovered.

The results for individual choice of rhythm.

In this series of records, the metronome was dispensed with, and the subject was permitted to react as he desired, taking the speed preferred because of ease, pleasure, or other reason.

Records were obtained for six subjects for feet, head, and hands,

both single-hand and double-hand movements, all for lengths of line 1 and 10 cm. The charts for individual choice were plotted for a comparison of speeds rather than for accuracy.

It was noted for the hands:

(1) that every subject reacts more rapidly with the left hand;

(2) the eyes had little effect as to changing the speed-rate;

(3) single and double hand-movements were equally rapid.

Some subjects, as A, react more rapidly for the shorter lines, though no clearly marked evidence of this speed-increase is to be noted.

For the head, the results for both eyes opened and closed show the impossibility of separating the optimal or preferred rate of speed on the score of visual assistance or because of direction of head-movement.

There is a close agreement of the subject as to his best speed, and this is independent of special conditions; for example,

A. selects 50–57 beats per minute for 1 cm. and 48–68 for 10 cm.; G. has a preference for 61–66 and 56–71; Hu. rises to 103–125 for 10 cm. and selects 68–82 for 1 cm.; Le. 52–55 for 1 cm. and 45–52 for 10 cm., and so on.

We may say, then, that free rate-choice for head-movements results in a selection of some rate of speed that is not affected by the visual sense or direction of movement, and is strictly individualistic, covering a range of 50–130 beats per minute, and not increasing as the amplitude of movement is reduced.

Turning to individual choice of speed-rate, for the feet it will be seen that

(1) the non-visual records closely correspond as to chosen speed, and there is a less close correspondence of visual speeds;

(2) the visual records are ruled at a lower rate in some cases, but A., G., and Mo. show little difference;

(3) there is a tendency to speed up as the series progresses;

(4) the shorter lines are ruled with greater speed as a rule, though G. and Le. fail to show this phenomenon;

(5) The left-foot records show a higher speed-rate for all cases.

Among many interesting points that cannot be examined in this connection, such as relation of voluntary choice of rate to the main line of metronome records as regards accuracy, the fact of the higher rate of ruling for the left hand and foot stands most prominent.

Whether a record of head-movements to right or left, or other devices to compare the sides of the body or to contrast arm and leg speeds, will bear out this testimony is as yet unknown, so that the

writer prefers to announce the result and not now fit theory to data. It may be said that the records were taken in reverse order and rearranged, as regards right and left foot or hand, and, in addition, the initial foot-movement varied with the subject, some being right and some left.

We ask finally: Is the time in which the greatest exactitude is produced, the same for every group of muscles; that is, has every motor apparatus the same natural rhythm? and: Is this natural rhythm a constant rapidity for all motor nerve-centres or does it depend upon the complexity and character of the movement?

The comparison will fall first on the averages and finally on the individual records.

The hand-movements show the following results:

Constant errors for 14 cm.:

For simple and weight accelerating and retarding motions, there is a close agreement about 120 beats for the minimum error for visual and right-hand non-visual records; left-hand non-visual records are spread more, but will also average the same.

For 10 cm. simple and compound movements the visual minimum errors are at 180–200 beats, while with the eyes closed the results are grouped about 60 beats; one record, that for l.h.e.c., has two minimum points at 60 and 180 beats, the latter being clearly a crossing of the 0 error-line, because of physiological limitations.

For 1 cm. simple and weighted minimum errors are grouped between 20 and 60 beats, while the simple and compound group show less regularity and a tendency to group minimum errors at 100 beats.

The head-movements show for both 1 and 10 cm. lines a minimum error at 180–200 beats, there being, however, one exception at 100 beats for 10 cm. head-backward movements.

The foot-movements show minimum errors at 80 beats for the right foot, and 180 and 100 beats for the left foot, visual and non-visual respectively.

Bearing in mind for a moment the individual choice records, there seems here a suggestion that the left foot is capable not merely of higher speeds, but of minimum errors at the higher rates as compared with the right foot.

No such differentiation of the hands can be discovered, however.

Mean errors:

For the hands:

For 14 cm. for simple and weighted results we find that the right-hand and left-hand eyes-open minimum errors are at 180 beats, but the non-visual left-hand minimums are at 30 beats.

For 10 cm. simple and compound records we find all minimum errors are between 160 and 200 beats.

For 1 cm. simple and weighted results there is a scattering of minimum errors from 20 to 200 beats, with a heavy preponderance at 200, and the same is true for the simple-compound series.

The head-movements minimum errors are at 40 beats without exception.

The foot minimum errors are distributed from 20–30 beats for the left foot to 160–180 for the right.

It is thus evident that each group of muscles and each motor centre has its own optimum, and that the conditions of complexity, resistance, etc., influence greatly the accuracy of the periodic movement impulse.

THE MOTOR POWER OF COMPLEXITY

BY C. L. VAUGHAN

A. Counting of Simple and Complex Visual Objects

If every sensory stimulus has a motor reaction, then a simple figure perceived in any way ought to produce a somewhat different response from a more complex figure similarly perceived. Of course if only one figure of each kind is given it is difficult to measure in any way this difference, since it is so small. But we might make it measurable by multiplying the process. Therefore I have cut out a row of similar figures in a strip of cardboard and on another strip another series of a different pattern. Now if these rows are counted figure by figure each figure has a certain motor effect which influences the speed of counting, so that the time of counting (measured by the chronoscope) should give some indication of the comparative motor power of the figures in question.

In the accompanying illustration nine cards of various patterns are shown. Cards 1, 2, and 3 are comparatively simple patterns while 4, 5, and 6 are comparatively complex, Card 6 having the added complication of different kinds of figures on the same card. Cards 7, 8, and 9 form another group, Card 7 having the same letter throughout, Card 8 having letters composing a sentence and Card 9 a series of the letters, mostly consonants, mixed promiscuously. In order to prevent the subject from knowing the exact number, and thus, perhaps, bring in another influence at the end of the row, most of the different cards have different numbers of figures, but this difference is not great and some cards have the same number. The subject usually forgets, from one experiment to the next, the number on each card.

At first the experiment was performed with the figures in a straight row, instead of in the broken line which is seen in the illustration. In counting the straight rows, the observers found it hard to keep the place in the line. A subject would become confused and count some spot twice or else he would omit it altogether. Furthermore this disturbance was found to be much greater with some figures than with

others, with Card 1, for example, more than with Card 2. Therefore the device was adopted of diversifying the line, both by placing some of the figures above and some below the line and by making the distances from one figure to the next, different in the different cases. And in order to prevent the subject from associating any peculiar turn in the line with a certain number counted, it was decided to have the arrangement on the different cards different. But it was still necessary

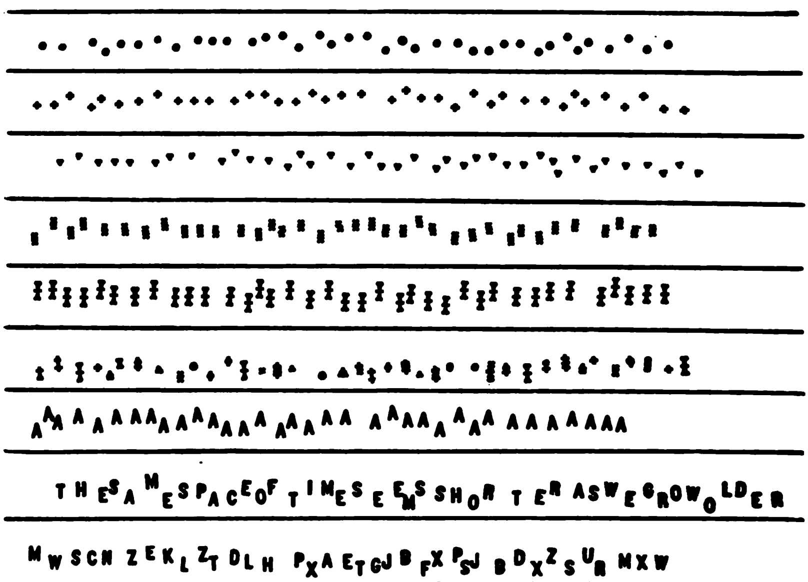


Fig. 1

to have the intervals between figures about the same in all the cards, and therefore the row was divided into sections of six figures each and these sections were used as units, variously arranged, in constructing the other rows. For example the first unit of Card 3 is the same as the second of Card 4. Sometimes this six-figure unit is turned end for end or upside down, and thus, though the same spaces are used, the cards appear dissimilar.

The subject would be seated at the table with one hand resting lightly on the key which sets the chronoscope in motion, his eyes raised so that the table in front of him is not seen. One of the cards would then be put in the proper position in front of him (always the same), and he is told that all is ready. He looks d[illegible] at the card and as soon as he begins to coun[illegible] [illegible]gures in [illegible] ; the chronoscope key, and when [illegible] re-

leases the key. The time for the operation is then noted. The whole series of cards is thus gone through. An extra card of which no record is taken is used for the first few tests so that the subject may be in the proper state when the first test to be noted down is taken. Also the order of the series is changed from one experiment to the next, each card taking its turn at being first and last. It was hoped in this way to distribute among the different cards the effects of practice and fatigue, and also to guard against any expectations on the part of the subject as to the character of the next card.

The subject is told to count as fast as he can, with a reasonable feeling of certainty as to his correctness, the main object being to have a uniform principle, in counting the different series. Wrong counts were excluded, but later on the same cards given again so as to keep the tables even. Subjects were not allowed to count the figures by groups, but one by one. At first a certain amount of difficulty was found in the fact that subjects in counting would repeat the numbers to themselves, and as they seemed to be retarded by this, especially in those numbers whose corresponding names have 2 or 3 syllables, the result was that we were getting the speed with which subjects could count the numbers from 1 up to 38 or 39 and this would be practically the same whatever the figure. But all the subjects were finally trained merely to think the number, or at least to have as little vocal adjustment as possible. When this was done the subject no longer felt that it was the speed with which he could count that was being measured but the rate at which he could take in the different figures on the card, one at a time.

Between three and four hundred tests were made of the counting of the figures on the nine cards, the work being divided among seven subjects, though not in exactly equal amounts. Since the number of figures on the different cards are different, I have found the time it takes to count one figure by dividing the total time by the number of figures on a card. The following table shows the average time taken by each subject for one figure on each card, time given in thousandths of seconds. *A.M.V.* stands for average mean variation.

	A.	*A.M.V.*	*B.*	*A.M.V.*	*C.*	*A.M.V.*	*D.*	*A.M.V.*	*E.*	*A.M.V.*	*F.*	*A.M.V.*	*G.*	*A.M.V.*
1	279.69	11.47	186.87	13.22	247.62	14.89	193.08	12.38	262.77	20.20	217.00	12.32	442.63	36.51
2	270.60	12.47	180.55	11.88	249.21	18.00	190.51	11.82	257.56	16.03	195.00	9.50	431.00	24.39
3	274.43	9.87	180.89	11.57	247.59	15.51	192.07	7.87	259.96	17.41	191.50	11.03	434.83	24.28
4	286.82	12.47	190.39	12.56	255.20	16.78	200.53	10.72	267.11	20.31	226.40	29.11	445.71	14.58
5	290.29	11.89	195.41	12.36	262.27	19.73	199.89	9.27	271.06	20.86	233.50	20.46	459.17	18.92
6	293.06	12.21	185.33	11.51	275.40	18.13	199.20	7.92	264.59	15.00	220.80	14.92	432.17	26.87
7	273.32	15.54	192.23	13.73	229.26	19.30	185.60	9.83	265.56	19.20	189.20	30.31	402.13	21.34
8	279.77	13.86	185.23	12.69	246.66	18.86	193.39	9.81	277.52	14.93	220.70	21.79	388.77	27.48
9	285.09	11.97	197 04	12.28	269.96	19.95	186.30	9.05	259.72	14.27	210.34	11,71	419.57	18.22

A, B, C, D, E, F, G are the different subjects, and 1, 2, 3, 4, etc., refer to the cards with the different patterns. It is seen at a glance that great differences exist between the rates with which the different subjects count. Subject G had much fewer tests than the others, and thus, not having as much training, his average is higher in comparison than it would be had he had the same training.

Now if we compare the counting of the first three or relatively simple patterns with that of the next three or comparatively complex ones, we notice at once that the simple figures are almost invariably counted in less time than the complex, there being only two exceptions. B counts 6 a little faster than 1, and G counts 6 faster than 1 and 3. Even these apparent exceptions are easily explained. As noted already, subjects are much more apt to lose their place in counting certain cards than others. This is especially true of Card 1 even after the line is broken. Now Card 6 is arranged on a different plan from the others, for it has many kinds of figures on it. This is a great help in keeping one's proper place in the counting of the series, and since wavering between two figures is avoided, the series is counted more rapidly. But B is the most rapid in counting, of all the subjects, and it is natural that any differences in the ease of keeping place should show themselves here, since the more rapid the counting the easier it is to lose the proper position. This cannot be said of G, who is a slow counter, but on the other hand it may be noted that he had only a few cases, and at first the ability to keep one's position is much less than after considerable experience. So in Cards 6 and 1 there are two conflicting principles, degree of complexity and tendency toward confusion of position. Of course both these principles are present in all the other cards, but they reach a maximum in 1 and 6, in 1 extreme simplicity with difficulty in keeping place, in 6 extreme complexity with ease in keeping place. Card 1, it will be seen, is with nearly all subjects a little slower than 2 and 3, while 6 is generally faster than 4 and 5.

Therefore it would seem that the apparently small exceptions are not real exceptions, but variations due to the presence of other factors than mere differences in complexity of the figures used. In observing the averages for 7, 8, and 9 we see that as a rule 7 is fastest, 8 next, and 9 the slowest. The tables are not quite so regular as for the cards just given. B and G count 8 faster than 7, and E counts 9 faster than 7. The most of these cards have on them 36, 37, 38, or 39 figures. Card 8 has 43 letters. The subjects report that the last three on this card are counted much faster. They know, as soon as they reach 40, just how many there are, and it is hard to keep from counting the rest in a

group. Otherwise they do not feel any difference in counting Cards 8 and 9. Arranging the letters in words does not affect the speed of counting, so far as they can see, for in counting they do not notice the words at all.

When we average the records of all the subjects giving equal weight to each subject, though the number of tests may be different with the different men, we get the following table. Time given in thousandths of seconds.

(1) 261.38
(2) 253.49
(3) 254.54
(4) 267.46
(5) 273.08
(6) 267.22
(7) 248.19
(8) 256.01
(9) 261.15

It is seen, from looking at this table, that all divergences from the general rule have stopped. Cards 1, 2, and 3 each take less time than any of the 4, 5, 6 group, and 7 is faster than 8 and 9. So the evidence seems very strong that it takes longer to count complex than simple figures. Should one object that the difference is extremely small, a few thousandths of a second, and that thus a slight error in one test might invalidate the result, we reply that the time which is given is the time in which we count just one figure of the given pattern, and that thus of course the difference between counting two different figures must be very small. Moreover there has been a remarkable agreement of the tests taken at different times. It is not a case of finding 1, 2, and 3 counted faster one day and 4, 5, and 6 counted faster the next, but 1, 2, and 3 are counted faster nearly every time. Occasionally 1 will take longer than one of the 4, 5, 6 group. And extremely seldom is there a case where the average of 1, 2, and 3 is not less than that of 4, 5, and 6.

The experiment seems to have proven that it takes a longer time to count a row of complex figures than a similar row of simple figures. *The complex figure exercises a retarding effect upon the eye as it sweeps along.* There is a greater amount of sensory stimulation, consequently a greater amount of motor excitement. This motor excitement does not act in harmony with the motor activity which impels the eyes along, but has a somewhat antagonistic effect. The eye is held more by the complex figure; it is a greater effort to withdraw the gaze to look at the next figure. A certain interest, as we say, on the psychological

side tends to hold one to the figure looked at. This interest is greater (other things being equal) the greater the complexity of the figure. The nervous processes involved in counting, though admittedly in very small degree, are thus inhibited by the complexity of the figure and act more slowly.

B. Reactions to Simple and Complex Optical Impressions

Since the preceding experiments seem to show that reactions on optical impressions are different according as the figures are more or less complex, it would seem that we ought to be able to measure by graphic methods the reactions to visual fields of varying grades of complexity and in this way to demonstrate their different motor powers.

A Porter kymograph was used on which to register the reactions. Resting on the top of the drum, and revolving with it, was a circular band of white paper, upon which were pasted the different figures to be observed. A screen was placed in front of the kymograph, thus concealing the figures; but at their level was a little square window in the screen, which, when the eye was placed in the proper position, allowed the subject to see one of the figures but nothing more. A few inches in front of this window was an eye-rest which kept the eye properly placed. A tambour received the movement from the subject and communicated it to a straw which made a scratch on the smoked paper which covered the drum.

The figures used in this experiment form two series, one, composed of geometrical figures, varying in complexity from a circle to a very complex figure consisting of many overlapping squares, triangles, etc., and the other composed of colored figures varying in complexity from a simple square of one color to a very complex mixture of various colors. The area of the visual field is about the same in all cases, — an inch square. The geometrical figures were formed of black lines on a white background. The figures used are shown in the accompanying illustrations.

The subject would be seated in front of the screen, his eye at the eye-rest a few inches in front of the window in the screen, and the forefinger of the right hand on the tambour, which is to the right of and behind the screen, and thus not seen while the eye is at the rest. Then as the drum revolves and brings a figure in front of the window, the subject observes this figure carefully, and when it is all in the field of vision he presses down with his forefinger, thus producing a curve on the drum surface. *He tries to make the same finger-movement every time*, whatever the figure at the window may be. But his attention is

not to be too much taken up with the making of the movement, for he must be closely observing the figure. If he looks at the figure until he observes its characteristics clearly and then turns his attention from this to the finger-movement, it is evident that the optical sensation would not have much effect upon the movement. The movement must be performed while his interest in the figure is highest. Now, after a little practice, any one can accustom himself to make a certain definite movement in about the same way every time, and he can then agree that he shall make this movement as a reaction to a given stimulation. Then when the stimulus comes he makes the movement without any longer thinking of the character of the movement. It has become, to a certain extent, automatic and can look out for itself.

This is the state into which I have tried to get my subjects. Their whole attention is to be taken up with the seeing of the figures in the window, and to these figures they are to react as automatically as possible. Thus, though finger-movements are usually voluntary, all the capricious character of voluntary action will be removed here, and if the stimulus is the same in all cases, the reaction tends to assume the form of a uniform movement. There is, then, a chance to see the influence of different optical stimuli upon this action.

Six different geometrical figures were seen at each revolution of the drum and six reactions given by the subject. Between figures a white surface would occupy the field of vision. The simple and complex figures were distributed so that the subject never knew what kind of a figure would come next. The purpose of the experiment was kept

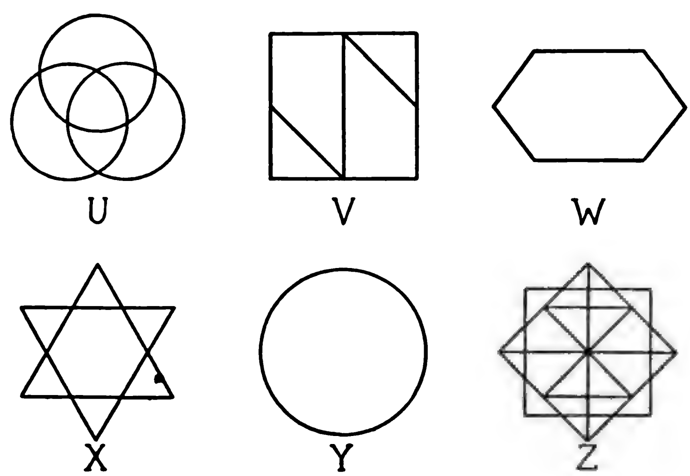


FIG. 2

as much as possible from the knowledge of the subjects; but some, knowing my general problem, surmised quite correctly my main object here.

Ten revolutions were made at each sitting, thus causing the subject to react ten times to each figure. Then a new drum paper was taken and the case with the colored figures placed upon it. This had five colored figures, and ten revolutions were made also in this case. Thus, in all, in any one day, the subject would make one hundred and ten of these finger-movements.

Since we have in all these experiments tried to find out in the different figures merely differences in the *amount* of the reaction, and not differences in the character of the reaction, we shall keep up this method here. Now a stronger reaction makes a higher curve, and since the drum is all the while revolving, and since the higher the curve, other things being equal, the longer it takes, the stronger reaction will also make a wider curve. So it would seem that if we wish to observe the differences in the amounts of reaction the most natural course to pursue would be to measure the heights and widths of the curves we have registered. This accordingly has been done.

In our discussion of these measurements let us, then, first, take up the curve heights, and of these, those of the geometrical figures which we call U, V, W, X, Y, Z. The height is measured from a base-line [drawn by revolving the drum after the subject has taken his finger from the tambour] to the highest point reached. These measurements are taken from two hundred reactions to each figure, divided among seven different subjects.

Heights of Curves

	U	*V*	*W*	*X*	*Y*	*Z*
Subject A	6.83	6.68	6.59	6.55	6.63	6.79
B	8.64	7.26	6.41	7.79	6.39	9.75
C	6.67	6.55	6.73	6.85	5.87	8.53
D	21.35	21.26	21.46	21.90	21.33	21.31
E	16.13	15.77	15.17	15.85	15.29	16.08
F	16.90	16.97	16.14	16.52	15.81	17.91
G	11.42	11.32	11.39	11.48	11.06	11.10
	87.94	85.51	83.89	86.94	82.38	91.48
Average	12.56	12.26	11.98	12.42	11.77	13.07

Arranged in order of height of curve

Z	*U*	*X*	*V*	*W*	*Y*
13.07	12.56	12.42	12.26	11.98	11.77

If we put the figures in the order of strongest reaction for the different subjects we get the following table:

Subject A	*U*	*Z*	*V*	*Y*	*W*	*X*
B	*Z*	*U*	*X*	*V*	*W*	*Y*
C	*Z*	*X*	*W*	*U*	*V*	*Y*
D	*X*	*W*	*U*	*Y*	*Z*	*V*
E	*U*	*Z*	*X*	*V*	*Y*	*W*
F	*Z*	*V*	*U*	*X*	*W*	*Y*
G	*X*	*U*	*W*	*V*	*Z*	*Y*

It is seen from these results that, although the subjects differ, the height of the curve varies directly with the complexity of the figure. The order of the figures, which we get by measuring the height of the curves and then putting that figure with the highest curve first, with the next highest second, and so on, is exactly the same order in which we should put them if we were asked to put the most complex first, the next second, and so on. Though the individual subjects may vary somewhat from this rule, when they are all grouped together there are no exceptions.

The variations of the reactions with the different subjects may be shown very clearly in the following way, where the different figures are in the left-hand side arranged in order of descending complexity. "1st place," etc., refer to the order of arrangement of the figures by the different subjects as shown in preceding tables. Thus, *Z*, 3 times, 1st place, means that three subjects have in the average a higher curve for *Z* than for any other figure.

	1st place	*2d place*	*3d place*	*4th place*	*5th place*	*6th place*
Z	3 times	2 times	0 times	0 times	2 times	0 times
U	2 times	2 times	2 times	1 time	0 times	0 times
X	2 times	1 time	2 times	1 time	0 times	1 time
V	0 times	1 time	1 time	3 times	1 time	1 time
W	0 times	1 time	2 times	0 times	3 times	1 time
Y	0 times	0 times	0 times	2 times	1 time	4 times

One can see at a glance from this, how, as the figures decrease in complexity, they take their position further on in the series. If a diagonal is drawn from the upper left-hand corner to the lower right, it will pass through or near the larger numbers in the table, thus showing that the figures belong in the ordered series in the places already shown.

Next in order let us take up the measurements of the widths of curves for the same geometrical figures which we have been considering.

Widths of Curves in mm.

		U	V	W	X	Y	Z
Subject	A	20.83	20.59	20.93	21.22	20.21	21.89
	B	11.18	10.77	10.46	10.31	9.92	10.79
	C	4.28	4.43	4.10	3.78	4.95	4.70
	D	21.08	19.36	18.33	18.75	18.17	21.09
	E	14.22	13.85	13.40	13.56	11.96	14.13
	F	17.00	15.26	15.92	16.52	14.52	16.47
	G	5.25	5.19	5.30	5.08	5.11	5.37
		93.84	89.45	88.44	89.22	84.84	94.44
Average		13.40	12.78	12.63	12.75	12.12	13.49
Order		*Z*	*U*	*V*	*X*	*W*	*Y*
		13.49	13.40	12.78	12.75	12.63	12.12

If as before we take the orders for the different subjects, we get the following table :

Subject	A	*Z*	*X*	*W*	*U*	*V*	*Y*
	B	*U*	*Z*	*V*	*W*	*X*	*Y*
	C	*Y*	*Z*	*V*	*U*	*W*	*X*
	D	*Z*	*U*	*V*	*X*	*W*	*Y*
	E	*U*	*Z*	*V*	*X*	*W*	*Y*
	F	*U*	*X*	*Z*	*W*	*V*	*Y*
	G	*Z*	*W*	*U*	*V*	*Y*	*X*

Here, as before, in the case of the heights, it is seen that though the order is different with the different subjects, yet the general tendency is to place the most complex figures first and the simplest last. The most simple figure *Y* never comes in front of the fifth place except with subject C, who places it first. This exception may be ascribed to the fact that this subject, on account of his going away, did not have so many tests. In fact only one day's work of 10 reactions for each figure is recorded, and it is but natural that some variations from the standard should occur in his case.

If now, as before, we investigate where each figure occurs in the series for the different subjects we get the following table:

	Times in					
	1st place	*2d place*	*3d place*	*4th place*	*5th place*	*6th place*
Z	3	3	1	0	0	0
U	3	1	1	2	0	0
V	0	0	4	1	2	0
X	0	2	0	2	1	2
W	0	1	1	2	3	0
Y	1	0	0	0	1	5

Here we again see the large numbers on a line from the upper left-hand to the lower right-hand corner.

Thus we get the following order from the geometrical figures as measured by the height and width of the curves:

Height	*Z*	*U*	*X*	*V*	*W*	*Y*
Width	*Z*	*U*	*V*	*X*	*W*	*Y*

The only difference, it is seen, is that the positions of *V* and *X* are reversed in the two series. Such a change would on our principle be fairly likely to occur, since *V* and *X* are figures near to each other in complexity and the motor effects are very similar.

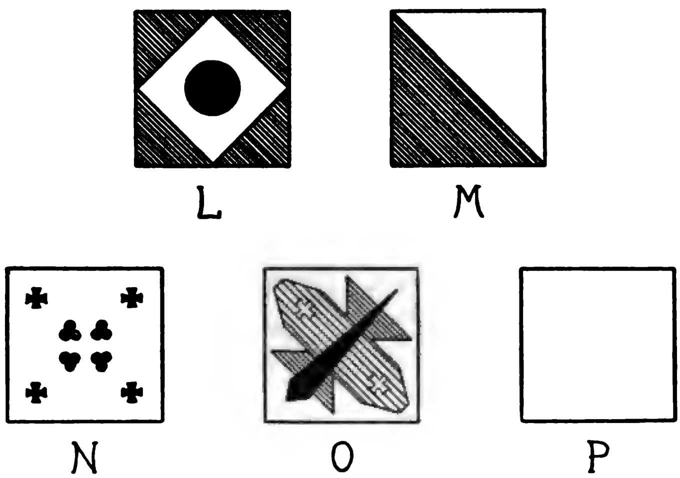


FIG. 3

In the same manner, the following tables show the reactions to the colored figures of different grades of complexity. And first, as before, is the table of the heights of the curves for the different subjects, given in millimetres. The numbers given represent the averages of all reactions made. We will call the figures, for the sake of reference, *L*, *M*, *N*, *O*, *P*.

	L	*M*	*N*	*O*	*P*
Subject A	5.75	6.01	5.90	5.82	5.74
B	6.72	5.56	6.35	7.53	4.94
C	10.92	10.90	10.76	10.52	10.99
D	25.49	25.42	26.23	25.89	25.52
E	20.63	20.82	20.37	20.55	20.30
F	15.67	15.23	15.15	15.98	14.51
	85.18	83.94	85.26	86.29	82.00
Average	14.20	13.99	14.21	14.38	13.67

Order arranged as before in a descending series according to height of curve:

O	*N*	*L*	*M*	*P*
14.38	14.21	14.20	13.99	13.67

This is exactly, as I should judge, the order of the complexity of the figures reacted to.

The arrangement by the individual subjects is as follows:

Subject A	*M*	*N*	*O*	*L*	*P*
B	*O*	*N*	*L*	*M*	*P*
C	*P*	*L*	*M*	*N*	*O*
D	*N*	*O*	*P*	*L*	*M*
E	*M*	*L*	*M*	*N*	*P*
F	*O*	*L*	*M*	*N*	*P*

We see that individual differences are stronger here than in the geometrical figures, but that the same tendency to react more strongly to the complex is present in nearly every case. This can be brought to the eye more clearly if we observe the table in which is shown the position of the different figures in the series of the different subjects.

	Times in				
	1st place	*2d place*	*3d place*	*4th place*	*5th place*
O	2	1	2	0	1
N	1	2	0	3	0
L	0	3	1	2	0
M	2	0	2	1	1
P	1	0	1	0	4

M here presents the principal exception, coming too often in the first place.

Finally we give the tables for the widths of the curves for the colored figures; and first the table of the averages of all the subjects for all the figures:

	L	*M*	*N*	*O*	*P*
Subject A	25.76	24.27	25.06	24.77	23.14
B	9.42	9.49	9.06	9.84	8.11
C	5.85	5.35	5.62	5.80	5.24
D	13.68	13.53	13.18	13.26	13.38
E	22.06	21.37	22.50	22.17	20.44
F	16.30	15.08	16.65	16.76	15.13
	93.07	89.09	92.07	92.60	85.44
Average	15.51	14.85	15.35	15.43	14.24

Order, arranged in a descending series according to width of curve:

L	*O*	*N*	*M*	*P*
15.51	15.43	15.35	14.85	14.24

Here the order is not just the same as we got from a measurement of the heights. The three complex figures have changed places somewhat, but there is no exchange of a simple and a complex.

The arrangements by the individual subjects are as follows:

Subject A	*L*	*N*	*O*	*M*	*P*
B	*O*	*M*	*L*	*N*	*P*
C	*L*	*O*	*N*	*M*	*P*
D	*L*	*M*	*P*	*O*	*N*
E	*N*	*O*	*L*	*M*	*P*
F	*O*	*N*	*L*	*P*	*M*

The three complex figures have different places with different subjects, but very seldom is a simple figure found among the complex, or *vice versa.*

This can be seen easily from the following table:

	Times in				
	1st place	*2d place*	*3d place*	*4th place*	*5th place*
L	3	0	3	0	0
O	2	2	1	1	0
N	1	2	1	1	1
M	0	2	0	3	1
P	0	0	1	1	4

A theoretical word may close our report.

The growth of biology and physiology has tended to show that there is no break in the nervous mechanism. The stimulus goes to the brain and out through motor channels to muscles, glands, etc. The nervous current does not wait in the brain for the permission of the mind to leave on its journey to a muscle nor does it need mental reënforcement. The nervous current as a whole is a unity. The nervous system is a physiological instrument for producing the appropriate reaction to a certain stimulus. In the unicellular organism there is no nervous system, but the protoplasm receives the stimulus and produces the reaction. As we go up in the animal series a differentiation is seen to be present in the organism. Some parts are more concerned with the receiving of stimuli and others with the approach toward or withdrawal from the stimulating object. There is a division of labor. The nerv-

ous system is developed as a means of rapid communication between the different parts, but this communication is a physiological one. The stimulus sets up a chemical action in the sensory organ which is transmitted along the nervous path to the motor organ which is caused to react. As we ascend the animal series the differentiation becomes greater and greater, and consequently the means of communication must become more and more complex. So trunk lines are formed which lead to a centre, and from this centre again go out main lines which divide and subdivide until the muscles are reached. The centre acts as a kind of automatic switch-board.

Accepting such a view of the nervous system it must be granted that different stimulations would produce different reactions. It was my aim in the experimental work which has been described to show that this is true. And while much work has already been done in showing that different kinds, or different amounts of stimulation produce differences in reactions, it seemed important to demonstrate also that mere differences in the complexity of the stimulus bring about differences in the reaction. So the experiment of counting figures of different complexity was entered upon, and we found that it took longer to count figures the more complex they were in spite of the fact that the act of counting seems always the same. The question is how must the fact that counting becomes slower and slower, as the figures become more complex, be interpreted?

When we count a row of figures, the eyes do not move along at a regular uniform rate, but make a quick jump from one figure to the next, halt a moment, make another jump, and so on. Now, I think the principal difference comes in with the figures of different complexity in the time the eye halts at each figure. The halt is longer the more complex the figure is. It is well known that any visual object which stimulates the retina is brought by a reflex movement of the eye to the place of clearest vision. Of two objects stimulating the eye at the same time, the more pronounced one will produce the reflex and will hold the eye longer than a weaker stimulus. Similarly here, the more complex figure produces a stronger reflex and holds the eye longer than the simple figure. This is repeated at every figure in the series.

The complex figures have more features about them, all of which by way of the retina and optic nerve are represented in the cortex and thus more cortical cells are involved, which in turn produce a stronger stimulation of the muscles which move the eye in the proper way to see the figure, and thus the eye is held more strongly by the complex than by the simple figure.

Again in the second experiment, the subject reacts more strongly to the complex as shown already in explaining the first experiment and for the same reasons. It might be said that in looking at the colored figures, *e. g.*, that since the same amount of retina is stimulated, the reaction ought to be the same. But we may presume that the complex figure, on account of the different shapes and contrasts on its surface, will more variously affect the same amount of retina and that the nervous currents sent to the cortex will, many of them, be stronger than those from the simple figure and will thus cause the cortical cells to be more strongly excited, or by a process of irradiation the stimulation will spread to adjoining cells and thus finally more cells be stimulated. However this may be, the amount of discharge into motor cells is certainly greater and the muscular reaction, therefore, also greater.

The interesting side of our results is thus given in the fact that we have here two activities — counting with highest speed and making hand-movements of certain length — which are performed every time with exactly the same intention and with the subjective impression of equal result, and which yet show marked differences according to the complexity of the psycho-physical stimuli. It is a new contribution to our knowledge of the independent motor power of ideas.

ANIMAL PSYCHOLOGY

THE MUTUAL RELATIONS OF STIMULI IN THE FROG RANA CLAMATA DAUDIN[1]

BY ROBERT M. YERKES

I. Animal Behavior and the Senses

Since the behavior of an animal is conditioned by its senses, it is extremely important that the comparative psychologist should have accurate and detailed knowledge of the sense-impressions received by his subjects. Knowledge of the so-called "special senses" does not suffice for the satisfactory description of behavior, for there are several other kinds of sense-data of equal or even greater importance than those of the five special senses. As investigation of the subject progresses the banefulness of the notion that all sense-experience is summed up in "the five special senses" becomes more and more evident. For the comparative psychologist the senses are not five, six, eight, or ten, but as numerous as are the kinds of sense-data which condition animal activities. There can be no doubt that many of the lower animals are largely dependent upon senses which are not included in the conventional special sense list. The contention that certain organs which are commonly recognized as sensory in function, the cristæ acusticæ of the ear, for instance, are merely reflex control organs, has little weight in this connection, for every sense-organ is part of a motor control mechanism, and so far as we are able to judge from available evidence each has as an accompaniment of its functioning a mode of sensation. If there are two kinds of peripheral organs in connection with the afferent nerves, namely, those whose functioning has sensation for an accompaniment and those in which motor control is the sole phenomenon, it is high time that the fact were definitely known.

[1] The results brought together in this paper have been published in part in connection with other work in the following papers: Inhibition and Reënforcement of Reaction in the Frog, Jour. of Comp. Neurol. and Psychol., vol. 14, p. 124, 1904. Bahnung und Hemmung der Reactionen auf tactile Reize durch akustische Reize beim Frosche, Arch. f. d. ges. Physiol., vol. 107, p. 207, 1905. The Sense of Hearing in Frogs, Jour. of Comp. Neurol. and Psychol., vol. 15, p. 279, 1905.

Even a thoroughly accurate knowledge of the general condition of the senses in a particular phylum, genus, or species may be of trifling value in the study of the behavior of a given individual, for within these groups the state of development and relative importance of a sense may differ strikingly. Herrick,[1] in his admirable investigation of the sense of taste in fishes, has rendered comparative psychology an important service by showing that even a highly developed sense may be of markedly different value in the associative life of different species. The cat-fish, according to Professor Herrick's observations, obtains its food primarily by the aid of taste-impressions, the hake by the aid of touch, and the sea-robin chiefly by means of vision. All three of the senses mentioned are possessed by each of the fishes, yet their values differ so widely that an understanding of the habits and associative processes of any one of the species would be impossible except in the light of just such facts as Herrick has discovered. Clearly, then, we must know the relative importance of the various sense-impressions received by an animal before we can discuss its behavior or psychic characteristics intelligently.

Furthermore, if behavior is to be serviceably described in terms of stimuli and physiological conditions it is necessary first of all to recognize that an animal responds to a situation, not to any one independent and isolated stimulus. Every situation, to be sure, may be analyzed into its component simple stimuli, but the influence of each and all of these stimuli is conditioned by the situation. Too often in our accounts of an animal's behavior we name some one stimulus as the condition of the reaction and entirely neglect the situation, without which the stimulus would have been of quite different value to the animal. For any given stimulus other external and internal stimuli constitute an environment. The complete description of a reaction demands knowledge of all the stimuli which enter into the situation and of their mutual relations of interference or supplementation. A frog which in its native habitat and undisturbed by an unusual situation would react violently to the light touch of a stick may give no sign of reaction to the same stimulus when a human being stands nearby. The influence of the tactual stimulus has been changed entirely by the simultaneous appearance of visual, olfactory, and possibly still other sense-data (man). The animal reacts not to the touch alone, but to this stimulus as part of a certain situation. The general effect of a situa-

[1] The Organ and the Sense of Taste in Fishes, Bulletin U. S. Fish Commission for 1902, pp. 237–272.

tion we often speak of as excitement, timidity, etc. These are words for which must be substituted in our accounts of animal behavior accurate descriptions of the situations. Experimental studies prove that an animal must become thoroughly accustomed to the general situation in which it is to be observed before the influence of any particular condition can be studied to advantage.

Only a few of the important reactions of an animal to either external or internal stimuli are visible to the casual observer, and many of them can be detected only by the employment of indirect methods. Frequently the lack of a visible motor response to a new situation is good evidence of a fundamentally important reaction. The death-feigning opossum, crustacean, or insect, truly reacts by becoming motionless. As Whitman[1] has shown in the case of the leech it is as hazardous to judge of the degree of sensitiveness of another animal solely on the basis of our own as it is to maintain that lower animals possess only the senses which are ours. Varied, indirect and delicate methods are necessary in the investigation of the senses, as the results of the experiments to be described below help to prove.

It is my purpose in this paper to call attention to and emphasize the importance of studying stimuli in their mutual relations of interference and supplementation. This I shall do by presenting the results of an investigation of the behavior of the green frog. I shall discuss briefly, first, the sense-data received by the animal, their relative importance, significance, and mutual relations, and, next, the phenomena of reënforcement and inhibition.

II. The Sensory Reactions of the Green Frog

The following sensory reactions have been observed in the frog, but most of them have not been studied with care: olfactory, temperature, visual, tactual, equilibrational, and auditory. It is my purpose to investigate each of these senses in such fashion that we shall know the receptive capacity of the animal. Thus far I have completed only the work on auditory reactions, but the chemical and temperature senses and vision will be discussed in similar fashion later.

At present there is little known concerning the chemical senses. Unpublished observations made by Mr. Sherwin in this laboratory indicate the existence of olfactory sensitiveness to camphor, iodine, and several other strong stimuli. The reactions to the stimuli were slow, however, and there is no reason to believe that the sense of smell

[1] Animal Behavior, Woods Holl Lecture Series, p. 300, 1899.

is of great importance to the animal. Of taste barely more is known than that it is present.

There is marked sensitiveness to variations in temperature, as I have demonstrated by preliminary test experiments, but the limits, distribution, and significance of this sensitiveness remain to be investigated. I am not aware that the existence of temperature spots has been determined. In connection with a study of the reactions of frogs to light Torelle [1] discovered that the animals suddenly become inactive and usually attempt to bury themselves when brought into a temperature of 8° to 10° C. This reaction is prompt and definite; its value to an animal which hibernates is evident, yet one would scarcely anticipate the suddenness and regularity with which it occurs.

My studies of habit-formation and reaction-time [2] have revealed the importance of vision in the life of the frog. Perception of movement appears to be of far greater value to the animal than perception of form or color. The spectral colors are discriminated in all probability, for the animals react very differently to those of the blue end than to those of the red. According to Torelle blue is preferred to red. There is evidence that red has a higher stimulating value than blue, and the apparent avoidance of red in Torelle's experiments may be due to this fact. None of the work with which I am familiar demonstrates that the suspected color-reactions are due to stimulation of the eye. They may be due to stimulation of the skin, for Parker [3] has shown that the reactions of *Rana pipiens* to light are due to stimulation of the sink skin
as well as of the eyes, or they may even be due to intensity instead of color.

The tactual-auditory sense series is better known and also, it would appear, better developed than the chemical series. A large portion of the body surface of the green frog is keenly sensitive to mechanical stimulation, and Steinach [4] by measurement of electrical changes in the nerves of the skin has discovered the existence of "touch spots." His method, which is ingenious, promises to be of considerable value in the objective investigation of the senses, but it involves operations on the subject which inevitably destroy the normal condition of the sense.

[1] The Response of the Frog to Light, American Journal of Physiology, vol. 9, p. 476, 1903.

[2] The Instincts, Habits and Reactions of the Frog, Harvard Psychological Studies, vol. 1, p. 590, 1903.

[3] The Skin and the Eyes as Receptive Organs in the Reactions of Frogs to Light, American Journal of Physiology, vol. 10, p. 31, 1903.

[4] Ueber die electromotorischen Erscheinungen an Hautsinnesnerven bei adaequater Reizung, Archiv für d. ges. Physiologie, vol. 63, p. 503, 1896.

According to Steinach we have in the negative variation in the electrical condition of nerves during stimulation a phenomenon which may be used in the determination of the threshold of stimulation as well as in the investigation of irritability. In a previous paper [1] I have discussed the associational rôle of tactual impressions as well as the tactual reaction-time. All my observations lead me to believe that touch is a highly developed and important sense in the green frog.

Of the senses intermediate between touch and hearing that of equilibration has been most discussed. Certainly there is good reason to suppose that the sense-organs of the semicircular canals of the ear furnish the animal with impressions of position, movement, and possibly also of direction. Further study of the tactual-auditory senses of frogs may indicate the existence of conditions similar to those discovered by Parker in certain fishes, in which, as he remarks, "the skin, lateral line organs and ears represent, figuratively speaking, three generations of sense-organs. The oldest is the skin stimulated by varying pressures, such as are produced by irregular currents, and capable of initiating equilibrational responses. From the skin have been derived the lateral line organs stimulated by water vibrations of low rate, and also significant for equilibration. Finally, from the lateral line organs have come the ears stimulated by water vibrations of a high rate and important for equilibration. The ear, unlike the skin and lateral line organs, is differentiated for its two functions, the sacculus for hearing, the utriculus for equilibration." [2]

The sense of hearing remains to be considered. My attention was first drawn to this subject by failure to obtain motor reactions to sounds in the investigation of the time-relations of the neural processes of the green frog. Although a large number of sounds of different qualities, pitches, and intensities were employed, no visible motor reactions were observed. This led me to seek the significance of what appeared to be either a surprising lack of sensitiveness to changes in the environment which would naturally be expected to stimulate the animal, or an interesting and important case of the inhibition of reaction to auditory stimuli. This suggested the question, Are frogs deaf, or do they under certain conditions completely inhibit their usual reactions to sound?

In the literature on the senses and reactions of frogs I have found

[1] Harvard Psychological Studies, vol. 1, p. 592, 1903.

[2] Abstract of paper read before Section F of American Association for the Advancement of Science in Philadelphia, 1904. Science, vol. 21, p. 265, 1905. See also Bulletin of the U. S. Fish Commission for 1902, pp. 45–64, and the same for 1904, pp. 183–207.

nothing which contributes importantly to our knowledge of the sense of hearing. Most of the investigations which deal with the ear are concerned with the equilibrational and orientational functions of the labyrinth organs, and have nothing whatever to say about hearing. In the natural histories the existence of a well-developed sense of hearing is usually assumed, and numerous instances of what are supposed to be reactions to sound are cited. It is to be noted, however, that none of the observations in these popular works furnishes satisfactory proof of the exclusion of the influence of visual stimuli. Among the few references to frog audition of which I have knowledge, the only one which seems worthy of special notice is that of Gaupp in his Anatomie des Frosches. Since his few paragraphs sum up the state of our knowledge on the subject, while at the same time furnishing an illustration of the assumption of hearing on the basis of analogy, I present the substance of them in free and slightly abbreviated translation.

"The labyrinth organ has an acoustic and non-acoustic (static) function. For these two functions, according to the leading if not generally accepted view, entirely different portions of the organ are in question, and since the non-acoustic is attributed to the three *Cristae acusticae ampullarum* and the three *Maculae* (*M. recessus utriculi, M. sacculi, M. lagenae*), there remain for the acoustic function only the *Papilla basilaris* and the *Macula neglecta*. It is not certain, however, that the non-acoustic organs do not participate in the acoustic function.

"With regard to the acoustic sense of the frog nothing exact is known. That it exists, and that in good development, is certain. The existence of the drum and columella, and the fact that frogs have a voice are unmistakeable proofs of hearing. The participation of the *Papilla basilaris* in acoustic functions is rendered certain by comparative anatomical studies: the *Papilla basilaris* is the nerve end-organ from which, in the mammalia, the undoubtedly acoustic organ of Corti arises. From analogy of structure we may also infer an acoustic function in the *Macula neglecta*: on this, as on the *Papilla basilaris*, there is a simple tectorial membrane, and further the *Pars neglecta*, like the *Pars basilaris*, has a strong thick wall which only in a limited region, namely, where it approaches a part of the perilymphatic space, is markedly thinner. (For fish Breuer (1891) has already stated that if they really hear — which is proved — the *Macula neglecta* alone can come into consideration in connection with the function, for there is no *Papilla basilaris* in fishes, and the six other nerve end-organs apparently serve the non-acoustic function.)[1]

[1] Anatomie des Frosches, VI, Lehre von Integument und von den Sinnesorga nen, pp. 751, 752, 1904.

As the green frog does not respond visibly to sounds under expermental conditions, I found it necessary to employ indirect methods in the study of audition. By observing the influence of sounds on respiration and on the reactions to certain electrical, tactual, and visual stimuli, I obtained results which, since they have already been described in detail elsewhere,[1] may be summarized here as follows:

1. Observation of frogs in their natural habitat shows that they are stimulated by sounds, but the sense of hearing apparently serves rather as a warning sense which modifies reactions to other simultaneous or succeeding stimuli than as a control for definite auditory motor reactions.

2. Experimental tests prove that sounds modify the frog's reactions to visual and tactual stimuli. When the sound accompanies the visual or tactual stimulus it serves to reënforce the reaction to the other stimulus, but when given alone it never causes a motor reaction.

3. The green frog responds to sounds made in the air, whether the tympana be in the air or in water. There is some evidence that the influence of auditory stimuli is most marked when the drum is half-submerged in water. The influence of sounds upon tactual reactions is evident when the frog is submerged in water to a depth of 4 cm.

4. Sounds varying in pitch from those of 50 to 10,000 vibrations per second affect the frog. The most striking results were obtained by the use of an electric bell with a metal gong. With this sound in connection with a weak tactual stimulus a maximum reaction may often be obtained even when either stimulus alone causes no perceivable reaction.

5. Sounds modify the reactions of the frog after tympana and columellæ are removed. Cutting of the eighth cranial nerves causes disappearance of the influence of sound. It is clear, then, that the reactions to sounds are really auditory reactions and that the sense of hearing in the frog is fairly well developed, although there is little evidence of such a sense in the motor reactions of the animal.

6. Experiments during the spring months show marked influence of sounds for both males and females, whereas experiments made during the winter indicate a much diminished sensitiveness to auditory stimuli in both sexes, but especially in the male.

[1] Journal of Comparative Neurology and Psychology, vol. 15, pp. 279–304, 1905.

III. The Mutual Relations of Stimuli

In studying the various influences of complication of stimuli in the frog, I have used two methods: the measurement of reaction-time and of the amount of reaction. The reaction-time results will be presented first.

Reaction-time to electric stimulation of the skin was studied with special attention to the influence of other stimuli which were given in definite temporal relation to the electric stimulus. A Hipp chronoscope, controlled by a Cattell's falling screen, served as a time-measuring apparatus. The other essentials of the apparatus were a reaction-box, and devices for giving the stimuli and indicating the reaction. On the bottom of the reaction-box a series of wires were so placed that an electric stimulus could be given to the frog resting upon them by the closing of a key in the hands of the experimenter. In preparation for each experiment the frog was placed upon these open circuit wires in such a position that the weight of its body pressed upon a delicate spring in the floor of the box, thus causing the chronoscope circuit to be completed. The forward jump of the frog in response to stimulation caused the breaking of this circuit by the release of the spring upon which the animal rested. When all was in readiness for an experiment the chronoscope was started, and a key closed which simultaneously gave an electric stimulus to the frog and completed a circuit which caused the chronoscope record to begin. The stimulus consisted of a current from one or more "Mesco" dry cells. The motor reaction of the frog broke the chronoscope circuit, thus causing the chronoscope record to stop. It was then possible for the experimenter to read from the dials of the chronoscope the time, in thousandths of seconds, intervening between stimulus and reaction (reaction-time). In case of additional stimuli in connection with the electric, various simple devices were introduced to meet the demands of the experiments. These will be described in connection with the statement of results in each case.

Electric and photic stimuli. A photic stimulus was given from one to two seconds before the electric stimulus by the turning on of a sixteen-candle-power incandescent light, which was placed thirty cm. in front of the frog in the case of one series of experiments and fifteen cm. above it in another. The light uniformly inhibited reaction to the electric stimulus, as is shown by the results of Table 1.

Table 1

Title of investigation, — Electric-Visual (Red Light).
Experimented on, — Green Frog No. 4.
Harvard Psychological Laboratory, — 9.40 A. M., Feb. 28, 1902.
Chronoscope control average, 189σ, — Electric stimulus, 1 Cell.

Number of Experiment.	*Reaction-time.*
No Light.	
1	152σ
2	145
3	221
4	327
5	263
6	271
7	329
8	215
9	225
10	216
Light before Electric Stim.	
11	No reaction.
12	No reaction.
13	No reaction.
14	No reaction.
15	No reaction.
No Light.	
16	216

The inhibitory influence of light depends upon the intensity of the electric stimulus. Even a very strong light will not cause much retardation of reaction to a three or four cell current. As the strength of the electric stimulus decreases the delay of reaction increases, until finally there is complete inhibition. At this point, an electric stimulus, to which the frog would react almost invariably when there is no disturbing condition, will fail to cause reaction in the presence of a sudden increase in light intensity.

Merzbacher [1] states that the leg reflex of a frog, so placed that its legs hang free in the air, is greater in response to a given cutaneous stimulus in darkness than in daylight.[2]

Electric and visual stimuli (moving object). For the purpose of determining the effect upon reaction-time to an electric stimulus of stimulation of the eye by a rapidly moving object, experiments were

[1] Ueber die Beziehungen der Sinnesorgane zur den Reflexbewegungen des Frosches, Arch. f. d. ges. Physiol., vol. 81, pp. 222–262, 1900.

[2] "Blendung oder blosse Lichtentziehung erhoht die Erregbarkeit für mechanische Reize" (p. 253).

made in which, as in the case of electric and photic stimuli, reactions to electric stimulus alone and to the visual and electric were observed alternately. Thus in the case of each pair of reactions it was possible to note whether the visual stimulus shortened or lengthened the reaction-time. The visual stimulus was given by quickly moving a finger before a window in the reaction-box.

Two series of twenty pairs of reactions each were taken with each of two frogs. In the first series the finger was suddenly moved across the window and the electric stimulus was given either simultaneously or a small fraction of a second later. It was impossible to arrange for accurate measurement of the temporal relations of the two stimuli in the case of these tests. In the second series the finger was moved back and forth before the opening in the reaction-box for an interval of at least a second before the electric stimulus was given.

These experiments, which were in the nature of preliminary tests, yielded the following results. When the stimuli were given almost simultaneously the visual reënforced the electric as was indicated by a shortening of the reaction-time. As appears in the upper part of Table 2, the average time of forty reactions, twenty for each frog, to the electric stimulus was 148^{σ},[1] and to the same stimulus when it followed the visual 128^{σ}. Furthermore, examination of the several pairs of reactions shows, as is indicated in the table, that there were twenty-seven cases in which the visual stimulus caused shortening of the reaction-time (reënforcement of the electric stimulus) to thirteen in which it caused lengthening (inhibition). When the visual stimulus preceded the electric by at least a second, the reaction-time to the electric stimulus was greatly lengthened. The averages are 150^{σ} for the electric stimulus alone, 178^{σ} when it was preceded by the visual. In this series there are twenty-five cases of inhibition to fourteen of reënforcement.

Electric and visual stimuli (moving red disc). The indications of the importance of the temporal relations of stimuli, so far as reaction-time results are concerned, furnished by these crude preliminary observations led to a more accurate study of the subject. A revolving disc, which moved at the rate of one revolution per minute, was so arranged that at a certain point it closed an electric circuit in which a magnet had been placed. This magnet attracted a steel arm at the end of which a disc of red cardboard 12 mm. in diameter was suspended. With the making of the circuit the steel arm was drawn downward suddenly and the red disc, by reason of the vibrations of

[1] Thousandths of a second.

the arm moved rapidly back and forth in front of a window in the reaction-box. In this way the moving object was exposed to view about ten cm. to the right and three cm. in front of the right eye of the frog. The revolving disc, a fraction of a second later, completed the electric stimulus circuit. Thus both stimuli were given automatically, at such an interval apart as the experimenter desired. In the two series of results now to be described the intervals were 0.1 and 0.5 second respectively.

Table 2

Reaction-time to Electric Stimulation Alone, and to the Same when preceded for 0.1, 0.5, or 1.0 Second by Visual Stimulus.

Frog.	Electric Alone.	Visual 0.1" before elect.	Number Inhibited.	Number Reënforced.	Number Equal.	Electric Alone.	Visual 1.0" before elect.	Number Inhibited.	Number Reënforced.	Number Equal.
Preliminary Series. Visual Stimulus Moving Finger. Averages for 20 reactions.										
No. 5.	179^{σ}	158^{σ}	6	14	0	163^{σ}	206^{σ}	14	6	0
No. 6.	116	98	7	13	0	136	150	11	8	1
Gen. Aver.	148	128	13	27	0	150	178	25	14	1
Visual Stimulus Moving Red Disc. Visual 0.1" before electric. Visual 0.5" before electric.										
Series I. Averages for 25 reactions.										
No. 5.	177	163	10	15	0	170	255	15	9	1
No. 6.	148	112	6	19	0	115	178	18	7	0
Series II. Averages for 25 reactions.										
No. 5.	135	120	7	18	0	155	259	24	1	0
No. 6.	128	111	6	19	0	132	227	17	7	1
Gen. Aver.	147	126	29	71	0	143	230	74	24	2

These series consisted of twenty-five pairs of reactions each, with two animals. The results of the series are presented separately, in the lower half of Table 2, because the experiments which constitute them were separated by a period of three weeks. It is to be noted that these results agree fully with those of the preliminary series. The visual stimulus of a moving red disc, given 0.1 second before a 2 cell electric stimulus, reënforces the electric reaction, *i. e.*, it shortens the

time of reaction. The same visual stimulus given 0.5 second before tends to inhibit the electric reaction, *i. e.*, it lengthens the time of reaction.

Tactual and auditory stimuli. Since in the frog auditory stimuli under experimental conditions seldom if ever cause visible motor reactions, the study of the influence of this mode of stimulation upon the reactions to other simultaneous or succeeding stimuli is of special interest. In the investigation of the relations of auditory stimulation to other forms of reaction *amount* of reaction instead of *reaction-time* was taken as a measure of the influence of the stimulus. By a method the details of which may be most easily understood by reference to the plan of the apparatus in Figure 1, the influence of auditory stimuli on the leg-movement induced by tactual stimulation was observed.

In these experiments the frog sat astride a wooden support, held in position by linen bands over the back and a wire screen cap over the head. The hind legs hung free, and any movement of one of them in response to a stimulus could be read in millimetres by reference to a scale on the wooden support. This method of measuring the value of a stimulus in terms of leg-reflex has been used by several investigators — most recently by Merzbacher.[1] I have found it desirable, as did Merzbacher, to observe the movements of a shadow of the leg on the scale and thus read the amount of movement, rather than to watch the leg itself and attempt to project it upon the scale.

As is indicated in Fig. 1, the auditory and tactual stimuli were given automatically by means of a swinging pendulum, *P*, which was held in position by the magnet *a* until released by the experimenter. Early in its swing the pendulum turned the key, *m*, thus completing a circuit which caused the auditory stimulus to be given; later in the swing the key, *n*, was turned, and the tactual stimulus thus given through the magnetic release of the lever, *l*. The interval between the auditory and the tactual stimuli could be varied from 0 to 2″ by changing the position of the key, *n*. For intervals over 1″ it was necessary to arrange this key so that the tactual stimulus was given at some time during the return swing of the pendulum.

The auditory stimulus used was either the sound of a quick hammer blow (momentary stimulus of Series I), or the ringing of an electric bell for a certain length of time (prolonged stimulus of Series II). In Fig. 1 the bell is shown. It was placed eighty cm. from the frog, and in order that the influence of vibration of the experiment table

[1] Arch. f. d. ges. Physiol., vol. 81, p. 227, 1900.

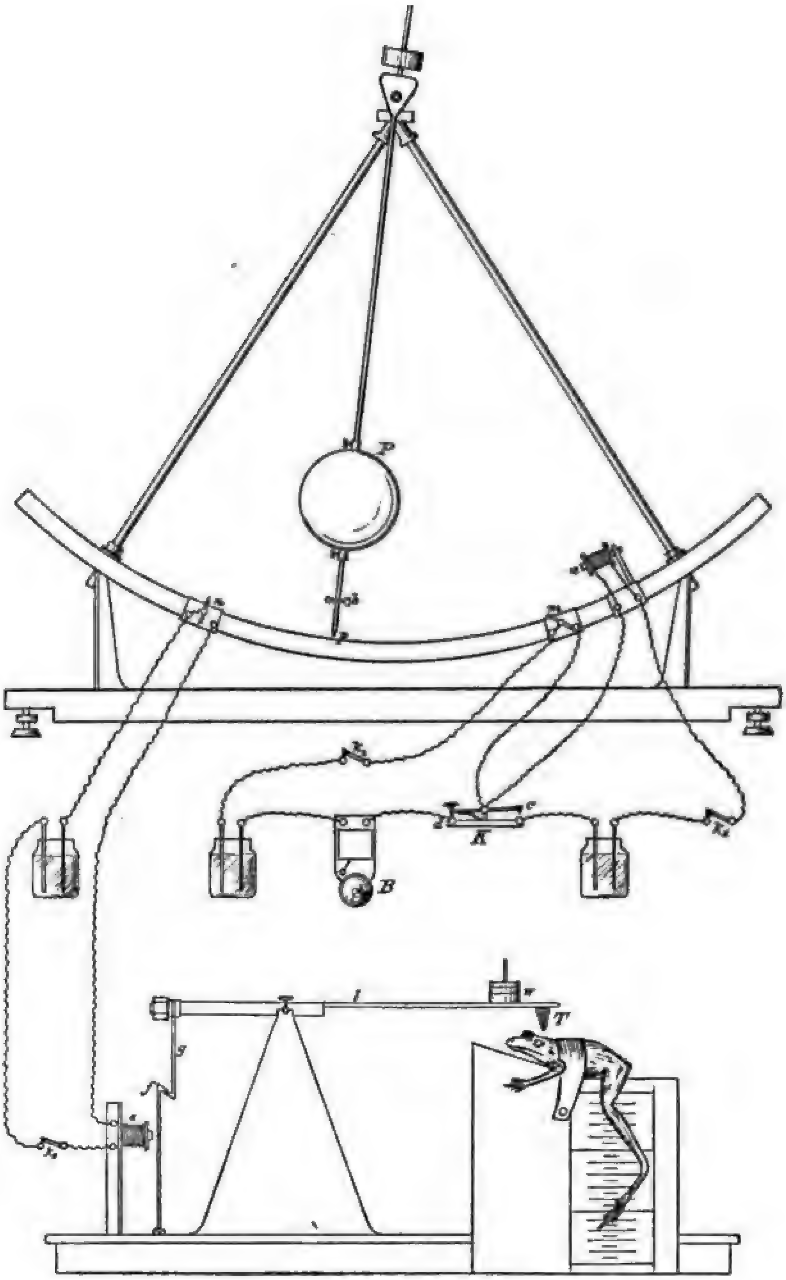


Figure 1. Auditory-tactual Reënforcement-Inhibition apparatus. *P*, pendulum; *p*, contact point of *P*; *b*, attachment for electro-magnet, *a*; *m*, key for circuit of electric bell, *B*; *n*, key for magnet circuit of tactual apparatus; *K*, hand-key for release of pendulum and temporary closing of electric bell circuit; k_1, k_2, k_3, keys in circuits; *e*, *f*, *g*, magnetic release for tactual apparatus; *l*, pivoted lever, bearing rubber cone, *T*, and weights, *w*. (Drawn by Dr. Wm. E. Hocking.)

might be avoided it was suspended from the pendulum frame. When the hammer was used it was placed sixty cm. from the frog, on the pendulum table. The holder for the frog and the tactual apparatus occupied a separate table which was not disturbed by the jars of the pendulum table.

The tactual stimulus was given by a rubber cone, *T*, two mm. in diameter at its apex. This rubber point, after the electric release of the lever to which it was attached, struck the frog at the middle point of a line drawn between the posterior margins of the tympana. The intensity of the stimulus could be varied by weighting the lever, *l*, at *w*.

All experiments were made with the green frog, *Rana clamata* Daudin. The reactions were taken regularly at half-minute intervals in pairs: first, a tactual stimulus reaction, then an auditory-tactual reaction. Ten, fifty, or one hundred pairs constituted a series. So far as the condition of the frog is concerned there seems to be nothing undesirable in long series, for fatigue does not appear, and so long as the animal is kept moist and in an unconstrained position, it continues to react normally, and without frequent struggles to escape. The advantage for the purposes of this investigation of taking the reactions in pairs, rather than taking separate series of reactions for each stimulus or combination of stimuli, is obvious. It enables us to compare directly the reactions of each pair, in other words those reactions which took place under most nearly identical conditions, and to note at once whether the auditory stimulus reënforced or inhibited the tactual reaction.

During a series the intensity of the tactual stimulus was changed as conditions demanded, but for any one pair of reactions it was always the same. It not infrequently happened that an intensity which at first caused merely a slight movement of the leg, later in the series uniformly brought about a maximal contraction, or the reverse might be true, and inasmuch as a maximal reaction to the tactual stimulus alone left no opportunity for judging of the influence of the auditory stimulus, when it was given in addition to the tactual, it was always necessary in such cases so to alter the intensity of the tactual stimulus that a medium reaction resulted.

The frogs, after being placed in the saddle-like holder and held firmly for a few seconds, seldom struggled very much, but if bound tightly they became irresponsive to the stimuli.[1] It was, therefore, necessary after they had quieted down to loosen the bands which held them in position. For the purpose of excluding the influence of visual

[1] A case of inhibition.

stimuli a wire screen cap covered with black cloth was put over the head; this served to keep the animal in position as well as to exclude visual stimulation.

a. Momentary auditory stimulation. Four frogs were used for a study of the influence of the momentary sound produced by a hammer blow, and for each of these animals fifty pairs of reactions were recorded in series each day. The temporal relation of the stimuli was changed daily during a week of experimentation: the results therefore consist of fifty pairs of reactions with each frog for each of the following seven intervals: (1) Auditory and tactual stimuli simultaneous, (2) auditory .25″ before tactual, (3) auditory .45″ before, (4) auditory .15″ before, (5) auditory .65″ before, (6) auditory .35″ before, (7) auditory .90″ before. The intervals were used in the experiments in the above order to avoid the formation of the definite habits of reaction which regular increase in the interval would have favored.

Typical of the results with all the animals are the following (Table 3) which were obtained with No. 1, a male. The figures in each case indicate the average of fifty reactions. Reënforcement and inhibition are expressed in terms of the tactual reaction, *i. e.*, the auditory-tactual reaction is so many per cent greater (reënforcement) or less (inhibition) than the tactual. In the tables reënforcement is indicated by the + sign; inhibition by the − sign. In the last column of the table is given the number of reactions that were reënforced or inhibited. This was determined by comparing directly the reactions of each pair. Cases in which the two reactions were the same were distributed equally between the two classes: tactual reactions reënforced by auditory stimulus, and tactual reactions inhibited by auditory stimulus. Assuming that the auditory stimulus was without effect upon the tactual reaction, the number of reactions in these two classes would be approximately the same, hence all auditory-tactual reactions over half in a series, *i. e.*, over twenty-five, which are greater than the corresponding tactual reactions, are reënforced reactions, and can be taken as a measure of the reënforcing influence of the auditory stimulus. In the same manner all reactions over half which show inhibition can be taken as a measure of the inhibitory value of the auditory stimulus.

As preliminary tests described in an earlier paper [1] furnished evidence of sex-differences, it is worth while to compare the results given by the males and females in these experiments with momentary auditory stimulation. For purposes of comparison I have presented

[1] Arch. f. d. ges. Physiol., vol. 107, p. 213, 1905.

in Table 4 the reënforcement-inhibition values given by the males and females for each interval. Column one contains the value of the auditory-tactual reaction in terms of the tactual reaction; column two, the number of reactions in excess of half which were reënforced or inhibited.

TABLE 3. FROG NO. 1. MOMENTARY AUDITORY STIMULUS, HAMMER BLOW. WEIGHT USUALLY 5 OR 10 GRAMS

Interval.	Reaction to Tactual Stim.	Reaction to Auditory and Tactual Stim.	Amount of Reenforc'm't or Inhibition.	Number of reactions Reënforced or Inhibited.
0″	6.84mm.	11.08mm.	+62.0%	+17.0
.15	22.22	28.96	+30.3	+17.0
.25	16.30	21.72	+33.3	+13.0
.35	24.90	25.32	+ 1.7	+ 0.5
.45	17.56	13.64	−22.3	−10.0
.65	17.46	15.72	−10.0	− 6.0
.90	31.26	31.48	+ 0.7	+ 0.5

TABLE 4. MOMENTARY AUDITORY STIMULUS, HAMMER BLOW

	Males Nos. 1 and 3.		*Females* Nos. 2 and 4.	
Interval	Per centum Diff.	No. of Reacts.	Per centum Diff.	No. of Reacts.
0″	+82.5% (Reënf't)	+17.5	+58.0%	+12.7
.15	+58.1	+17.0	+25.4	+ 8.5
.25	+32.3	+12.7	+39.8	+12.7
.35	+ 4.0	+ 1.2	− 9.7	− 3.2
.45	−13.5 (Inhibition)	− 7.2	−13.9	− 7.2
.65	−12.5	− 6.2	−11.8	− 7.2
.90	− 0.7	− 1.5	− 2.6	− 0.5

In these results two striking differences between the males and females appear: first, the reënforcement is not so great for the females as for the males; second, inhibition appears earlier and continues longer with the females than with the males. The average reënforcement with simultaneous stimuli is 82.5% for the males against 58.0% for the females. Inhibition begins to appear in case of the females when the interval between the stimuli is .25″ to .35″; in case of the males it appears between .35″ and .45″. Finally at .90″ interval inhibition is slightly greater for the females.

Although the exact significance of these facts is unknown, it is not improbable that they are indicative of fundamentally important sex-differences in reaction to sound. The males among frogs are usually

the vocalists, although in some species the females also croak. Moreover, in case of the green frog the tympanum of the male is much larger than that of the female. The results presented would seem to indicate that certain sounds stimulate the males to activity, whereas they inhibit activity in the females.

Graphically represented, the results of the momentary auditory stimulus experiments with frogs Nos. 1, 2, 3, and 4 are as follows:

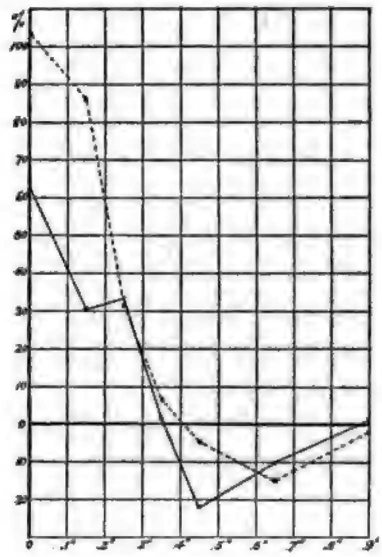


FIG. 2. Reënforcement-Inhibition curves for momentary auditory stimulation, based upon amount of reaction. Male No. 1 —— Male No. 3 . . . . .

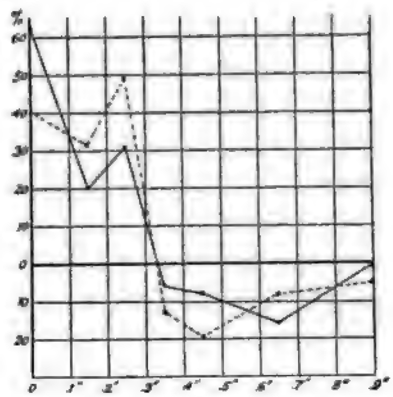


FIG. 3. Reënforcement-Inhibition curves for momentary auditory stimulation, based upon amount of reaction. Female No. 2 —— Female No. 4 . . . . .

The curves are all plotted by the method which will now be described in connection with Fig. 2. This figure presents the reënforcement-inhibition curves for the males No. 1 (solid line in the figure) and No. 3 (broken line). If in this figure we let the zero-point on the ordinates represent the value of the reaction to the tactual stimulus when given alone, then the value of the reaction to the auditory-tactual stimuli would be represented at some point above the zero-point if this reaction was greater than the tactual reaction (reënforcement), and below the zero-point if the reaction was less than the tactual (inhibition). Since one of our chosen measures of reënforcement and inhibition is the amount, in per cent of tactual reaction, by which the auditory-tactual reaction exceeds or falls short of the

tactual reaction, such a curve of reënforcement-inhibition as that of Fig. 2 (solid line) can be constructed at once from the data given in column four of Table 3. Here the auditory stimulus, when simultaneous with the tactual, caused 62 % reënforcement, as is indicated in the figure. The figures in the left-hand margin of the curves indicate amount of reënforcement or inhibition in per cent of tactual reaction; those at the bottom of the curves mark the intervals. On the

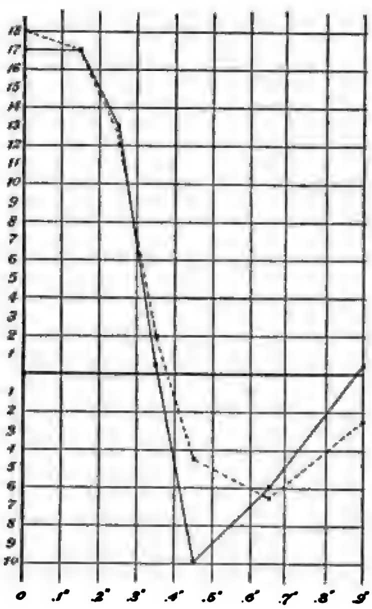


FIG. 4. Reënforcement - Inhibition curves for momentary auditory stimulation, based upon number of reactions. Male No. 1. —— Male No. 3 . . . . .

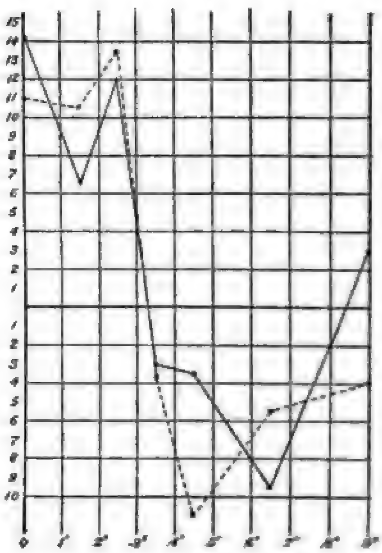


FIG. 5. Reënforcement - Inhibition curves for momentary auditory stimulation, based upon number of reactions. Female No. 2 —— Female No. 4 . . . .

curves dots indicate the intervals used in the experiments. Each of the curves is plotted on the basis of 700 reactions.

In every way comparable with the curves for the males No. 1 and No. 3 in Fig. 2 are those for the females No. 2 and No. 4 of Fig. 3. The similarity of the two curves in each figure is noteworthy. Inasmuch as the conditions of experimentation were the same for all the animals this would seem to indicate sex-differences which are worthy of further investigation. The curves show clearly the greater reënforcement in the males, and the greater inhibition in the females.

Figures 4 and 5 are the reënforcement-inhibition curves for the same

series of experiments plotted on the basis of the *number* of reactions in excess of half that were reënforced or inhibited. As there were fifty pairs of reactions with each frog for each interval, uniform reënforcement would be represented by twenty-five reactions above the base-line; uniform inhibition by twenty-five reactions below the base-line. The number of reactions is indicated by the figures in the left margin; the intervals, by those below the base-line. As an illustration of the application of the method of plotting, the curve for male No. 1

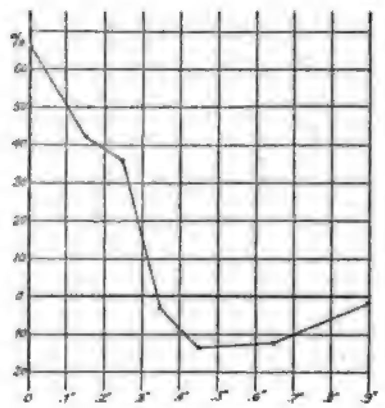


FIG. 6. Composite Reënforcement-Inhibition curve for momentary auditory stimulation, based upon amount of reaction. Frogs Nos. 1, 2, 3, 4. (Males and females.)

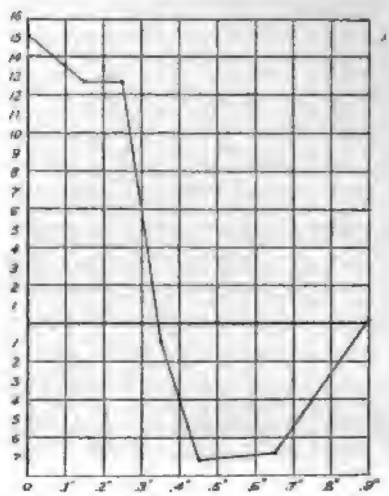


FIG. 7. Composite Reënforcement-Inhibition curve for momentary auditory stimulation, based upon number of reactions. Frogs Nos. 1, 2, 3, 4. (Males and females.)

(solid line) of Fig. 4 is constructed from the data of column five of Table 3. With simultaneous stimuli 17 reactions in excess of half, *i. e.*, 17 + 25, or 42, were reënforced; at .35″ interval .5 of a reaction was the average amount of reënforcement; at .45″ interval 10 reactions in excess of half, *i. e.*, 35, were inhibited, therefore the curve falls to 10 below the base-line.

Just as Figures 2 and 3 permit of direct comparison of the results of the measurement of the *amount* of reënforcement and inhibition for males and females, so Figures 4 and 5 make possible comparison in similar fashion of the *number* of reënforced and inhibited reactions for the sexes. It is to be noted that the two sets of curves, plotted on the bases of *amount* and *number* of reaction, agree in all important respects.

Figure 6 is the composite curve of amount of reënforcement-inhibition for the four animals; Figure 7 is the composite curve of the number of reactions reënforced and inhibited.

Summarily stated, the results of the experiments thus far described are: (1) The auditory stimulus of a quick hammer blow produces the maximum amount of reënforcement of tactual reaction when it is given simultaneously with the tactual stimulus; (2) as the interval between the auditory and the tactual stimulus approaches .35″ the amount of reënforcement gradually decreases; (3) when given .35″ before the tactual stimulus the auditory is practically without effect upon the tactual reaction; (4) as the interval increases above .35″ inhibition begins to appear; (5) the inhibitory influence of the auditory stimulus is greatest when the interval is about .45″; (6) when the interval is as long as .90″ the auditory stimulus is again ineffective. It thus appears that the reënforcement-inhibition curve of this particular stimulus under the conditions described is representative of a neural process which completes itself, in passing through two phases, a positive phase (reënforcement) and a negative phase (inhibition), in about one second.

b. Prolonged auditory stimulation. The experiments previously described have proved that a momentary auditory stimulus, which when given alone never produces a visible motor reaction, either reënforces or inhibits the reaction to a tactual stimulus which it accompanies or precedes. The experiments now to be described were made for the purpose of ascertaining whether reënforcement and inhibition occur in the same way if the auditory stimulus is prolonged, instead of momentary.

In a trial series of experiments with frog No. 1, one hundred pairs of reactions were recorded for each of six intervals of auditory stimulation. The auditory stimulus was given by the ringing of an electric bell. For all intervals the ringing of the bell continued until the tactual stimulus was given. When the two stimuli were given simultaneously the auditory stimulus was necessarily momentary, as in the foregoing experiments, but for all other relationships of the stimuli the bell rang for a certain length of time before the tactual stimulus was given. The six relations of the stimuli were: (1) simultaneous, (2) bell .2″ before and until tactual, (3) bell .6″ before, (4) bell 1.05″ before, (5) bell 1.5″ before, and (6) bell 2.0″ before. The other conditions of these experiments were the same as those previously described, except that the auditory stimulus was here given by the opening of the key which released the pendulum, instead of being given

by the turning of a key in the course of the pendulum swing. This method of giving the auditory stimulus as the pendulum was released was found unsatisfactory because of the irregularity of the magnetic release; at one time the pendulum would start immediately, at another time there would be a delay of as much as .1″

The reënforcement-inhibition curve plotted on the basis of the 1200 reactions in this series is presented in Fig. 8. Before stopping

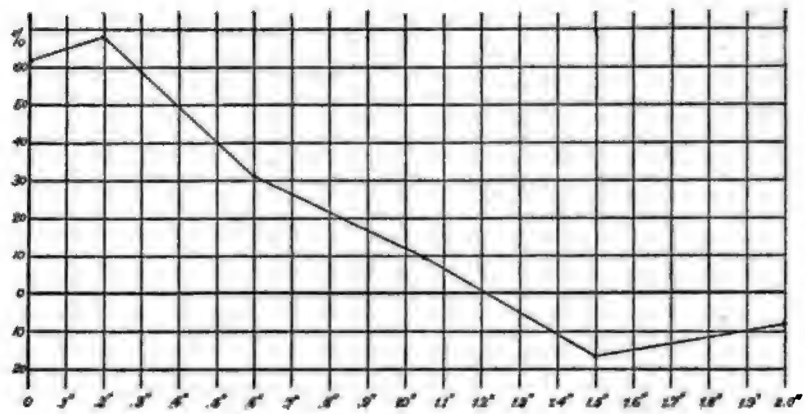


FIG. 8. Reënforcement-Inhibition curve for prolonged auditory stimulation, based upon amount of reaction. Frog No. 1.

to consider the important features of this curve we should note the results of certain more accurate experiments with prolonged auditory stimulation.

With two animals, No. 2, a female, and No. 3, a male, fifty pairs of reactions were taken for nine different intervals (see Table 5) of auditory stimulation. Each of the curves of Figures 9 and 10 is therefore based upon 900 reactions. The conditions for these experiments were the same as those for the momentary stimulation series, save that the electric bell took the place of the electrically actuated hammer, as the mechanism for auditory stimulation.

The important facts exhibited by the results of these prolonged auditory stimulation experiments in contrast with those with momentary auditory stimulation are: (1) That whereas for the momentary auditory stimulus of a hammer blow the reënforcement is greatest for simultaneous stimuli, in case of the prolonged stimulation with the electric bell, reënforcement increases during an interval of .25″ of auditory stimulation. Hence, the two conditions of stimulation give

us different types of reënforcement-inhibition curve. For the momentary stimulus the maximum reënforcement appears at simultaneity, and for the prolonged stimulus at .25″; (2) that the transition from reënforcement to inhibition occurs at 1.2″ in the prolonged stimulation curves, while in the momentary stimulation curves it occurs at .35″; (3) that the maximum inhibition which appears in the curves under discussion at about 1.5″ is less in comparison with

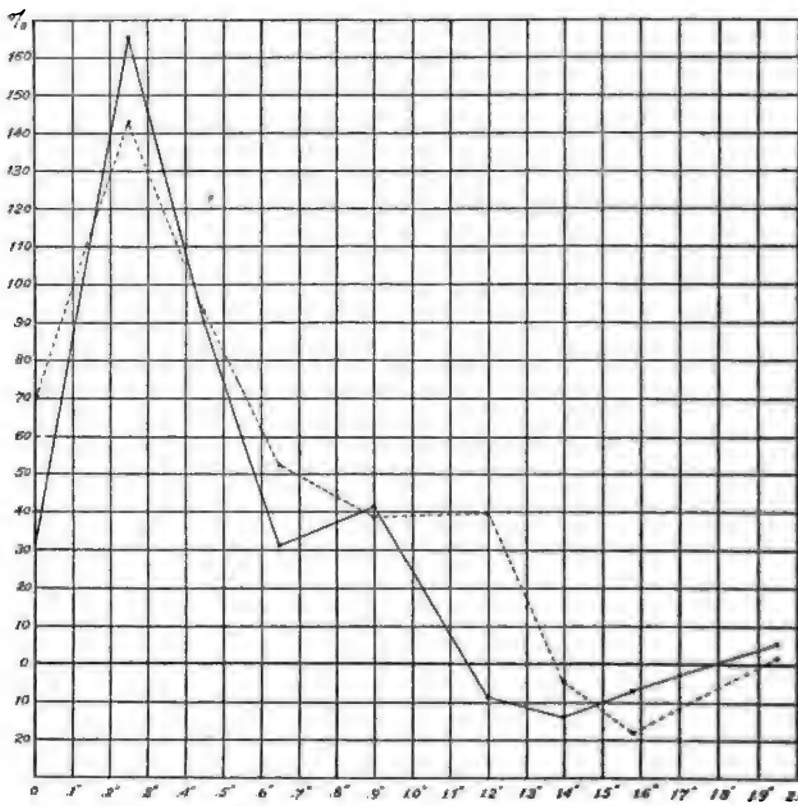


FIG. 9. Reënforcement-Inhibition curves for prolonged auditory stimulation, based upon amount of reaction. Female No. 2 —— Male No. 3 . . . .

the amount of reënforcement than that of the momentary stimulation curves; (4) that the auditory stimulus becomes ineffective when the interval during which it continues before tactual stimulation is 2.0″. The curves of Figures 8, 9, and 10 are then representations of a neural process which passes through a positive and a negative phase in about 2″. The effect of prolongation of the auditory stimulation interval

is to lengthen the period of reënforcement; the period of inhibition shows little modification.

For the purpose of showing in greater detail the nature of the results of this work the data from which the curves of Figures 9 and 10 were constructed are presented in the accompanying Table 5.

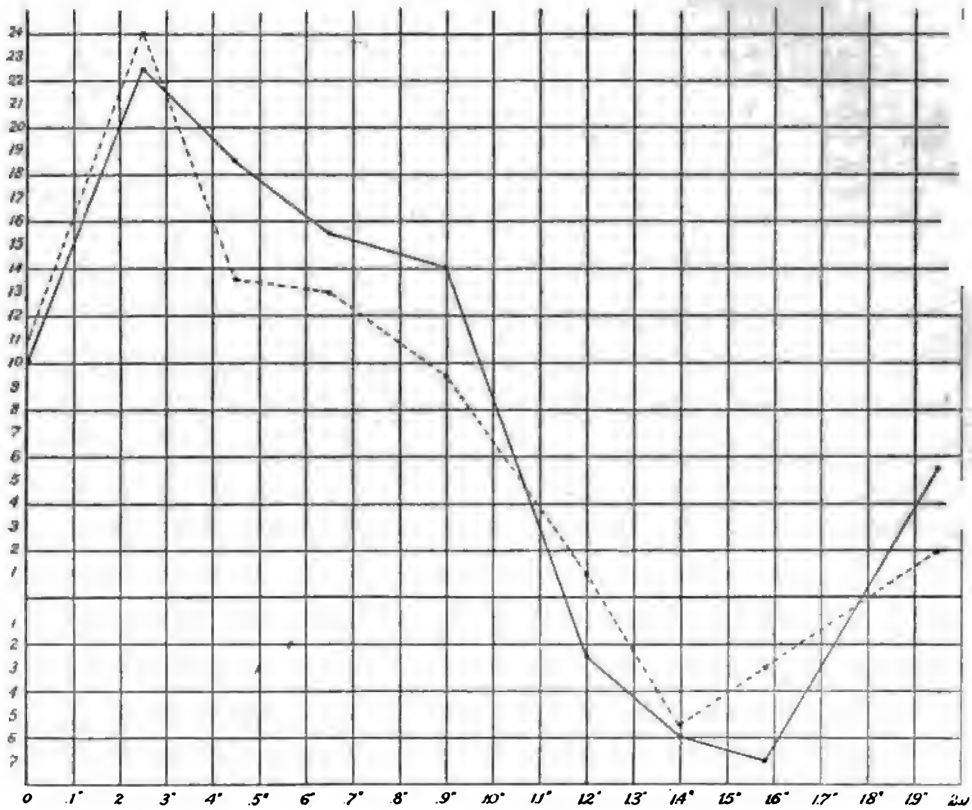


FIG. 10. Reënforcement-Inhibition curves for prolonged auditory stimulation, based upon number of reactions. Female No. 2 —— Male No. 3 . . . . .

Having now presented the results of my own investigation I wish to call attention to certain of their relationships to the work of other investigators, and to discuss briefly their significance.

TABLE 5. PROLONGED AUDITORY STIMULATION (ELECTRIC BELL)

Frog No. 2. Female. Weight usually 25 grams.

Interval.	Tactual Stimulation.	Auditory and Tactual Stimulation.	Amount of Reënforcement or Inhibition.	Number of Reactions, Reënforced or Inhibited.
0″	9.20 mm.	12.12 mm.	+ 31.7%	+10.0
.25	4.56	11.88	+160.5	+22.5
.45	8.94	16.94	+ 89.5	+18.5
.65	17.18	22.50	+ 31.0	+15.5
.90	9.42	13.32	+ 41.4	+14.5
1.20	10.54	9.64	− 8.5	− 2.5
1.40	24.00	20.64	− 14.0	− 6.0
1.58	19.16	17.80	− 7.1	− 7.0
1.95	14.50	15.40	+ 6.2	+ 5.5

Frog No. 3. Male. Weight usually 5 or 10 grams.

Interval.	Tactual Stimulation.	Auditory and Tactual Stimulation.	Amount of Reënforcement or Inhibition.	Number of Reactions, Reënforced or Inhibited.
0″	14.92 mm.	25.20 mm.	+ 68.9%	+11.0
.25	15.88	38.54	+142.7	+24.0
.45	13.48	26.02	+ 92.9	+13.5
.65	18.30	27.94	+ 52.6	+13.0
.90	20.94	29.06	+ 38.8	+ 9.5
1.20	21.90	30.58	+ 39.6	+ 1.0
1.40	19.18	18.34	− 4.4	− 5.5
1.58	32.24	26.30	− 18.1	− 3.0
1.95	13.86	14.14	+ 2.0	+ 2.0

VI. DISCUSSION OF LITERATURE AND RESULTS

The literature on reënforcement and inhibition is large, and even that portion of it which deals especially with the importance of the temporal relations of stimuli in connection with reënforcement and inhibition is so extensive that it does not seem worth while to attempt to give a systematic résumé of it for the purposes of this paper. I shall therefore call attention merely to those investigations which have contributed directly to the solution of the problems with which we are now concerned.

Bowditch and Warren [1] discovered that knee-jerk in the human subject is reënforced when an auditory, a visual, or a tactual stimulus precedes the tendon blow by .1″ to .5″, whereas the same stimuli have an inhibitory influence when they are given from .5″ to 1.0″ before the tendon blow.

At the suggestion of Bowditch, Cleghorn [2] undertook to investigate

[1] Journal of Physiology, vol. 9, pp. 60, 61, 1890.
[2] American Journal of Physiology, vol. 1, p. 336, 1898.

the influence of complication of stimuli upon voluntary movements. In this research graphic records taken in connection with an ergograph indicated (1) that "a sensory stimulus applied just as the muscle was beginning to contract (voluntarily) caused an increase in the height of the contraction, and (2) that the relaxation following a contraction with intercalated sensory stimulus is quicker and more complete than when no stimulus is given (p. 344). Cleghorn did not give special attention to the significance of the temporal relations of the stimuli which he employed, and his work was limited to the phenomenon of reënforcement of voluntary action by reason of the appearance, during the progress of his research, of an excellent paper on the interference of stimuli by Hofbauer.[1]

Hofbauer covered thoroughly the ground which Cleghorn had planned to work over. The ergographic method was employed also by Hofbauer in his very careful study of the interference of impulses in the central nervous system of man. It was noticed that while the subject was rhythmically contracting a certain group of muscles in response to some prearranged signal (*e.g.*, the sound of a metronome) the report of a pistol caused the contraction which immediately followed it to be much greater than the average of the rhythmic series, while the next contraction was correspondingly less than the average. It thus appeared that the sudden sound caused, first, reënforcement of the voluntary movement, then, inhibition. The reënforcement is greatest, according to Hofbauer, when the voluntary movement occurs immediately after the pistol report. When the report precedes the metronome signal by .2″ reënforcement is still marked, but thereafter it decreases rapidly in amount, until finally at .5″ inhibition appears. When the interval between the two stimuli is 1.0″ the first stimulus has practically no effect upon the voluntary movement in response to the second. (Hofbauer, p. 558.)

What Bowditch and Warren, not to mention other students of the subject, have described for reflex action in man, Hofbauer, Cleghorn, and others have shown to hold true also of voluntary movements. Unfortunately my own investigation was completed up to the point of the writing of this paper before I read Hofbauer's work, so I have not followed methods of dealing with my data which would make our results directly and easily comparable. But, whatever may be the relations of our results in detail, there can be no doubt that what he has demonstrated for man is true in its important aspect of the reënforcement-inhibition phenomena for the frog.

[1] Arch. f. d. ges. Physiol., vol. 68, p. 546, 1897.

Important in their bearings upon the phenomena of reënforcement and inhibition which we are now considering, are the various studies of refractory period and rhythm of nerve cell and fibre. The existence of a refractory period in neural substance, similar to that demonstrated for certain kinds of muscle by Marey,[1] Englemann,[2] Kaiser,[3] Cushny and Matthews,[4] Woodworth,[5] and many others, has been proved by Broca and Richet.[6]

Broca and Richet found that in the normal dog the refractory period of the nerve substance is too short to be easily detectable, they therefore experimented with animals which were lightly chloralized and kept at a temperature of 30 to 34° (the mean normal temperature of the dog is about 39.5°). Under these conditions a dog, when two identical stimuli (quality and intensity the same) were applied to the cerebral cortex successively, exhibited the following reactions: (1) When the stimuli were separated by .01″ they reënforced one another (addition); (2) when the interval was .1″ they inhibited the reaction partially (subtraction).

Concerning this phenomenon Richet writes in his dictionary of physiology (p.5): "Marey showed, in 1890, that the heart of the frog, at certain moments of systole, was inexcitable. Now our experiments prove that the cerebral apparatus, a certain time after the excitation, also ceases to be excitable: it then has a refractory phase, and this refractory phase is much more prolonged than that of the cardiac muscle." In a later publication Richet[7] makes the somewhat startling statement that a refractory period is not exhibited by the nerves of cold-blooded animals. In the tortoise, according to his results, reënforcement occurs so long as the interval between the two stimuli is not greater than 2″, while for longer intervals each stimulus to all appearances works independently. Richet seems to have generalized from a study of the tortoise. That his generalization is unwarranted seems to me highly probable in the light of the results of this paper, for there are many reasons for supposing that the reënforcement-inhibition phenomena with which we have been dealing in case of the frog are manifestations of the existence of the same process in the nervous system which under somewhat different conditions of experimentation exhibits itself in the so-called refractory period.

[1] Travaux du Laboratoire de Marey, 1876.
[2] Arch. f. d. ges. Physiol., vol 59, p. 309, 1894.
[3] Zeitschr. f. Biol., vol. 32, p. 1, 1895.
[4] Journal of Physiology, vol. 21, p. 213, 1897.
[5] American Journal of Physiology, vol. 8, p. 213, 1902.
[6] Comptes rendus, vol. 124, p. 573, 1897.
[7] Nature, vol. 60, p. 629, 1899.

The researches of Richet and his students indicate that the time of the process which conditions the phenomena of reënforcement-inhibition is about .1″. Stimuli given at .1″ intervals do not interfere with one another. That the process underlying the refractory period and the reënforcement-inhibition phenomena of our experiments is a rhythmic double-phase process is made still more probable by the following results. Horsley and Schäfer [1] found that the rate of response of the monkey to cortical stimulation was 12 per second, and Schäfer [2] discovered that the maximum rate of volitional impulses in man is 10 to 12 per second.

It was shown by Exner that certain movements of the foot of a rabbit could be produced by stimulating either the cortex or the skin of the foot. Simultaneous stimulation of both regions gives reënforcement. Stimulation of the cortex, if given not more than 3″ before subliminal stimulation of the skin, renders the latter effective. When both stimuli are subliminal each makes the subsequent one effective if the interval between them is not over $\frac{1}{8}$″ (Schafer [3]). Similarly for the dog Exner [4] proved that cortical and cutaneous stimuli reënforced one another, when both were subliminal, if the interval between them was not greater than .6″. Cortical and auditory stimuli, and auditory and cutaneous (of the skin of foot) gave similar results.

Physiologists have long been familiar with several aspects of the phenomena of reënforcement and inhibition in the frog, but I know of no detailed study of the significance of the temporal relations of stimuli in this connection. Goltz [5] called attention to the inhibition of the croaking reflex by peripheral stimulation, as well as to several similar phenomena. Nothnagel,[6] Lewisson,[7] and Wydensky [8] further contributed to our knowledge of the interference effects of stimuli in the frog. Wydensky proved that the application of an induced current to a nerve-muscle preparation may result in either contraction or relaxation of the muscle, according to the frequency of stimulation.

More recently Merzbacher [9] has dealt with the influences of com-

[1] Journal of Physiology, vol. 7, p. 101, 1886.

[2] Journal of Physiology, vol. 7, p. 111, 1886.

[3] Text-book of Physiology, p. 841, London, 1900.

[4] Arch. f. d. ges. Physiol., vol. 28, p. 495, 1882.

[5] Beiträge zur Lehre von den Functionen der Nervencentren des Frosches, p. 41, Berlin, 1869.

[6] Centralb. f. d. med. Wissensch., vol. 7, p. 211, 1869.

[7] Arch. f. Anat., u. Physiol., p. 259, 1869.

[8] Arch. d. Physiol. norm. et pathol., vol. 4, p. 690, 1892.

[9] Arch. f. d. ges. Physiol., vol. 81, p. 222, 1900.

plication of stimuli in the frog with the purpose of ascertaining the relations of the sense-organs to the reflex movements of the animal. His first paper is concerned especially with the functional importance of the eye in connection with reflexes. Unfortunately for the demands of this research, he did not attend particularly to the temporal relations of his stimuli. That a visual and a cutaneous stimulus were given either "at the same time or within a short interval of one another" (p. 250) is not the sort of information our problems demand.

According to Merzbacher's very interesting results a visual stimulus reënforces the reaction to a cutaneous stimulus. As the results of this paper show, this is only half a truth, for the two stimuli may either reënforce or inhibit one another's reactions. As Merzbacher observed no evidences of reaction to auditory stimulation he presumably did not attempt to study the influences of the ear in connection with reflexes.

There can be no doubt that the words reënforcement and inhibition as at present used in connection with the functions of the nervous system cover a multitude of widely differing phenomena. We can at once distinguish at least two important kinds of reënforcement or inhibition: first, that which is due to the functioning of special augmentary or inhibitory portions of the nervous system; second, that which is the result of the complication of stimuli. Any and every process in the nervous system may have either a reënforcing or an inhibiting influence upon simultaneous or succeeding processes; doubtless most processes or impulses at various times have both effects. The nervous system is constantly being modified by impulses from many sources, which suppress or strengthen one another according to their relative intensity, their temporal relations, and the motor relations of the portions of the organism which they affect.

The existence of the so-called refractory period in brain cortex and nerve indicates that every stimulus causes certain fundamentally important changes in the condition of the neural substance. These changes we may for convenience of illustration describe as modification of excitability, or of the functional capacity of central or peripheral tissues. Every stimulus causes a portion of the neural substance to pass from its normal state through a condition of increased excitability, which we may designate the positive phase, to a condition of diminished excitability, the negative phase. There is first an increase in the functional capacity of the tissues, then a decrease. If during the course of the change produced by a given stimulus a second stimulus becomes effective its result in reaction is determined by the particular phase of the tissues upon which it intrudes. If the nervous system is

in the condition of increased excitability, and the two stimuli act upon sensory regions whose motor connections are not antagonistic, the reaction will be reënforced, as we say, by the previous stimulus; if, however, the second stimulus falls upon the negative phase of the nerve substance, the reaction will be partially or totally inhibited.

The facts which are most prominent as the result of this investigation are, first, that the temporal relation of stimuli is an important condition of certain forms of reënforcement and inhibition; second, that the interference effects of two stimuli cannot be studied to advantage without attention to the relations of the forms of reaction which are appropriate to each stimulus.

V. Summary

1. Motor reactions of the green frog to electric stimuli are inhibited either partially or wholly by photic stimuli. The visual stimulus of a moving object has a like effect. It has been found, furthermore, that the same visual stimulus may either inhibit or reënforce the motor reaction in response to electric stimulation. When the two stimuli are given simultaneously reënforcement occurs, when the visual stimulus precedes the electric by half a second or more inhibition appears.

2. An auditory stimulus, which does not produce any visible reaction when given alone, modifies respiration and the reactions to other stimuli when given in connection with them.

3. The momentary auditory stimulus of a quick hammer blow when simultaneous with tactual stimulation reënforces the reaction to the latter stimulus. This reënforcement, or increase in the amount of reaction, ranges from 50 to 100 % of the average reaction to the tactual stimulus alone. When the auditory stimulus is given before the tactual reënforcement occurs in gradually decreasing amount until the interval between the two stimuli reaches .35″; at this point the auditory stimulus has no apparent effect upon the tactual reaction. As the interval is still further increased inhibition appears and continues for intervals between .35″ and .9″. Reënforcement is greatest when the two stimuli are simultaneous; inhibition is greatest when the momentary auditory stimulus precedes the tactual by .4″ to .6″. When the interval reaches .9″ the first stimulus does not affect the reaction to the second.

4. Reënforcement is greater for the males than for the females; inhibition appears sooner and lasts longer in case of the females. This apparently indicates that the males are stimulated to activity by certain auditory stimuli, whereas the females are rendered passive by similar sounds.

5. Prolonged auditory stimulation by means of an electric bell causes reënforcement and inhibition, according to the temporal relations of the stimuli, as does momentary auditory stimulation, with the following differences: The maximum reënforcement occurs when the tactual stimulus is given about .25″ after auditory stimulation has begun; reënforcement continues for a period of 1.2″, *i. e.*, when the electric bell continues to ring until the tactual stimulus is given, it reënforces the tactual reaction from simultaneity to 1.2″. Inhibition then appears, and continues until 1.8″. Both momentary and prolonged auditory stimulation cause first reënforcement, then inhibition of the appropriate reaction to a tactual stimulus.

6. The reënforcement-inhibition curves for the frog are very similar to those for man.

7. In case of the several pairs of stimuli whose interference effects have been studied reënforcement-inhibition appears. The first stimulus reënforces reaction to the second so long as the interval between them is not more than about .4″, while it inhibits the reaction when the interval is longer. Whether this reënforcement-inhibition curve as given in the experiments described may similarly be obtained for any and every pair of stimuli, no matter what their relation to reactions, remains to be determined.

8. In connection with the study of the mutual relations of stimuli of which this paper gives an account certain facts concerning the sense of hearing have been discovered. A summary statement of the results on hearing may be found on page 551.

THE TEMPORAL RELATIONS OF NEURAL PROCESSES

BY ROBERT M. YERKES

MUSCLE contraction-time, according to the determinations of several investigators, varies about .0035″.[1] Sanderson states that the time for direct stimulation of the muscle is approximately .0035″ and for indirect stimulation, by means of the nerve, .007″. The rate of nerve-transmission in the frog ranges from 25 to 35 metres per second.

Reflex reaction-time, as might be expected, varies widely with the nature of the reaction elicited by a stimulus, the condition of the animal, and the quality and strength of the stimulus. For many of the simple motor reactions of the frog it ranges between 20 and 60^{σ}.[2] Whether reflex reaction-time is to be sharply contrasted with instinctive and voluntary reaction-times, or whether they indistinguishably merge into one another is a question of considerable interest and importance for the student of the evolution of activity.

Voluntary reaction-time may be as short as 150^{σ} or as long as life, in an animal capable of profiting by experience as does the frog. It is preëminently the delayed type of reaction-time.

So much concerning the temporal relations of neural processes in the frog being well established, the purpose of the present paper is to call attention to some experimental results which indicate the existence of clearly defined types of reaction, and suggest possible values of reaction-time as a sign of mind.

The specific problems to be considered are: (1) Do reaction-times, in any given animal, range with equal frequency of occurrence from short to long, or are there certain modes (most frequented classes) which indicate definite types of reaction, such, for example, as the reflex, instinctive, etc.? (2) If there is distribution of the reaction-times about one or more modes, what are the types of reaction indicated

[1] Sanderson: Journal of Physiology, vol. 18, p. 147. Tigerstedt: Archiv f. Physiologie, p. 111, 1895. Boruttau: Archiv f. Physiologie, p. 454, 1892.

[2] σ = thousandths of a second.

thereby? (3) Finally, is reaction-time of service as a sign or measure of consciousness?

I wish especially to call attention to the fact that this paper deals with the reactions of the frog, not with animal reactions in general.

Reactions to Electrical Stimulation and Types of Reaction

Two years ago in connection with a discussion of the reaction-time of the green frog to electrical and tactual stimuli,[1] I presented a curve showing the distribution of 277 reaction-times to an electrical stimulus. The curve exhibited two clearly defined modes: one at between 60 and 70^σ and the other at about 160^σ. There was further a group of delayed reactions ranging about 500^σ. This form of distribution was interpreted, at the time, as indicative of three types of reaction, called, respectively, the reflex, the instinctive, and the delayed.

I have since obtained and examined with reference to form of distribution the further data which are presented in this paper. The reactions are all those of the green frog to electrical stimulation. The stimulus was applied by means of wires on the reaction-board on which the frog rested during the experiments. When reaction occurred in response to the electrical stimulus a circuit through the time-measuring apparatus was broken by the release of a delicate spring which had been held in place up to the instant of reaction by the weight of the frog. A Hipp chronoscope, controlled by a Cattell falling screen, served as a time-measuring mechanism. Three intensities of stimulus were used: (1) A current from one Mesco dry cell, (2) from two cells, and (3) from four cells.

Of the reactions whose time was measured there are three series. Series I is constituted by the recorded reaction-times in response to a one-cell stimulus, Series II, those in response to a two-cell stimulus, and Series III, those in response to a four-cell stimulus. The number of reactions, range and mode of each series are as follows:

	Number of reactions	Range	Mode
Series I	193	$161 - 798^\sigma$	235^σ
Series II	288	$41 - 647$	235
Series III	256	$61 - 178$	105

The distribution of the 481 reaction-times of Series I and II is shown by Figure 1; that of the 256 reaction-times of Series III, by Figure 2. For both of these distribution polygons the reaction-times were arranged in ten^σ classes, beginning with the class $41–50^\sigma$ [2] in the

[1] Yerkes: Harvard Psychological Studies, vol. 1, p. 609, 1903.

[2] The last four classes of Fig. 1 are 100^σ classes, 401–500, 501–600, 601–700, 701–800.

case of the combined Series I and II and with the class 61–70$^{\sigma}$ in the case of Series III.

Series I exhibits a primary mode at 235$^{\sigma}$. There are no reflex reactions in this series, unless it be maintained that the reflex reactions of the frog may have a reaction-time of over 160$^{\sigma}$, but there are a number of delayed reactions, some of which have reaction-times as long as 798$^{\sigma}$. This intensity of stimulation (one cell) may be said to call forth prompt reactions, which we may provisionally call instinctive, and delayed reactions, which have all the appearances of voluntary acts. There are no reactions which come within the range commonly considered as the reflex range of the frog (20–60$^{\sigma}$), and there are relatively few delayed reactions: almost all centre about the mode 235$^{\sigma}$.

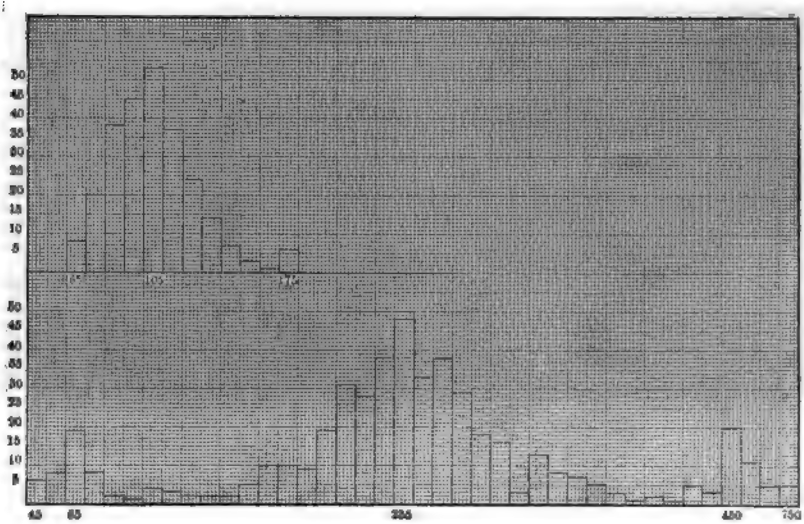


FIG. 1, LOWER. FIG. 2, UPPER.

Series II, in contrast with Series I, exhibits a secondary mode at 65$^{\sigma}$ in addition to the primary mode at 235$^{\sigma}$. The stimulus-intensity of this series (roughly twice as great as that for Series I) induces a variety of short reaction, which did not appear in the case of the one-cell stimulus, and at the same time fewer delayed reactions. The range of the reaction-times for the two series is about the same, but the lower limits are markedly different.

Observation of the subjects during the experiments revealed two methods of reaction to the two-cell stimulus : a locomotor reaction

(jump) which at once removed the animal from the source of stimulation, and a twitch of the hind legs which was instantly followed by the above-mentioned locomotor reaction. The leg reactions constitute the reflex group of Fig. 1, the usual prompt locomotor reactions, the instinctive group, and the slow locomotor reactions, the delayed or voluntary group.

It is to be noted that the instinctive reaction-time mode is the same for the two intensities of stimulation. This apparently indicates that change in intensity of stimulation causes a change in the type of reaction, not merely a gradual change in the position of the mode. For example, the modal reaction-time of 235$^\sigma$ given by a one-cell stimulus did not shift to 200$^\sigma$ or lower, as might have been expected, but instead there appeared a new type of reaction. The average reaction-times for the two series indicate a decrease in time with increase in intensity of stimulation, but they give no indication of the really important difference in the two series of reactions. The great importance of the distribution of the data, in addition to the common statistical quantities, is manifest.

Series III, whose reactions occurred in response to a very strong stimulus, differs in several important respects from the other series. Its range is much narrower, only 117$^\sigma$. Delayed reactions are lacking, and so also, curiously enough, are the reflex reactions of Series II. Instead of either or both of the modes of Series II, there appears in Series III an intermediate mode at 105$^\sigma$.

Our interpretation of these facts is facilitated by results of observation of the reacting subject. The leg reflex which frequently occurred in response to the two-cell stimulus never appeared in response to the four-cell stimulus. This in part explains the lack of the short reaction-time mode of Series II; it does not, however, account for the lack of delayed reactions. The latter fact may be referred to the intensity of the stimulus. Another difficulty in interpretation appears in connection with the intermediate mode, 105$^\sigma$. Is this to be considered an instinctive mode, as were those at 235$^\sigma$, or a reflex mode? Where is the line between reflex and instinctive action to be drawn? These results very clearly indicate that no line can be drawn, except quite arbitrarily. Reflex reaction-time, in the case of the frog, is continuous with instinctive, yet for any given situation the reflex, instinctive, and delayed (voluntary?) modes are likely to appear, as, for example, in the case of the data of this paper. Our conclusion must be, therefore, that although types of reaction are indicated by reaction-time results, the mode for a given type varies too much in position with different

conditions to make it possible to say that a particular reaction-time is that of a certain type.

We may safely say, then, that for any given subject, the muscle contraction-time, nerve transmission-time, and simple sensory reaction-time to the constant stimulus in question being known, we should be able safely to interpret reaction-time records in terms of reaction types. For reflex, instinctive, and voluntary are terms which designate modes of reaction, albeit not isolated classes, for they intergrade.

Whether there are more types of reaction than are indicated by the data of this report does not concern us at present, for the practical as well as the theoretical bearings of our conclusions depend upon the existence of types, and not upon their number.

Reaction-Time as an Indication of Consciousness

Hesitation in reaction is commonly accepted as an important sign of volitional consciousness in man; consequently delayed reactions in lower animals are supposed to be indicative of psychic processes. Granting this much, reaction-time may be used as a sign of consciousness. It cannot be denied that the longer the reaction-time of a given animal the greater the probability that the reaction is conditioned by mental processes. Such a statement, it is true, has a basis neither better nor worse than that of most of our inferences concerning the nature of the actions of our fellow beings. As I have already attempted to show in a discussion of criteria of consciousness in animal psychology,[1] there is no one criterion of consciousness which can be used alone satisfactorily, but instead there are numerous signs of mind each of which has value according to the number and variety of our observations concerning its occurrence in connection with states of consciousness. The more of such signs we discover and learn to evaluate properly in relation to consciousness in its different grades and to one another, the safer will be our inferences concerning the existence of mental processes in animals.

Reaction-time is presented in this paper as an additional sign of mind. Like all other signs it is of value only if used as one of a series of indications of mental life. For if we attempt to judge of consciousness by reference to reaction-time alone, we may be seriously misled, whereas if we use it in connection with docility, variability, neural specialization, and other recognizedly valuable signs, we may be greatly aided in our inference. As in juristic procedure judgment is not

[1] Yerkes: Journal of Philosophy, Psychology, and Scientific Method, vol. 2, p. 143.

based upon one bit of evidence nor even upon the evidence of a single witness, but upon evidence accumulated from all available sources, so in our attempts to judge of the existence of consciousness, it matters not whether the being be human or infra-human, we should make use of all phenomena which are recognized as signs of mind. The chief task of comparative psychology at present is the discovery and evaluation of signs of mind.

Reaction-time data, however, furnish another sign, or, as I prefer to call it in this case, measure of the intensity of consciousness; for variability of the time of reaction as well as its duration is significant. Reflex reaction-time is relatively constant, instinctive varies considerably, and the variability of voluntary reaction-time is extremely large. Degree of variability of reaction-time may be used as an indication of consciousness in the same way that variability in the form of reaction is used. The higher the power of consciousness the greater the variety in form of reaction and the variability of the reaction-time.

Reaction-time studies, as well as introspection and the investigation of animal behavior, indicate the importance of three activity concepts: automatism, instinct, and will. The automatic act is quick and relatively constant in form as well as reaction-time, while all signs lead us to infer that consciousness, when it accompanies the act, is a sequent phenomenon and not a condition of the act. The instinctive act is both slower and more variable in form and time than the automatic: consciousness is indicated as an accompaniment, and apparently it is at times a condition of the act. The will-act is extremely variable, unique in form, and almost without limits of reaction-time, for the conscious organism may react to the present situation in a fifth of a second, a day, or a year. Will is experience in action: it is our name for individually acquired control, and voluntary action is above all consciously conditioned activity.

Reaction-time, with respect to its two aspects of duration and variability, may be used as a sign or criterion of consciousness, for in accordance with the nature of these two sets of facts we classify acts as reflex, instinctive, or voluntary.

THE MENTAL LIFE OF THE DOMESTIC PIGEON

AN EXPERIMENTAL STUDY OF CERTAIN EMOTIONAL AND ASSOCIATIVE PROCESSES

BY JOHN E. ROUSE

I. INTRODUCTION

NATURALISTS have observed the habits of pigeons, and physiologists since Flourens have subjected them to numerous experiments, but so far they seem to have received little psychological study. As a contribution to this interesting field the present paper reports an investigation of certain emotional and associative processes of the domestic pigeon. Since the literature of the subject is meagre, I shall state at the beginning a few related facts which I have gathered from various sources; then I shall discuss in detail the problems, methods, and results of my several experiments.

The brain of the pigeon is well developed, although the hemispheres are unconvoluted. When they are removed, the animal retains unaltered its reflex and vital activities, but ceases for a time at least to show evidence of mental life, for example, memory and will.[1] In the normal animal sight and hearing are acute, and touch seems keen, although the claws are not used for grasping and eating, as in the case of more intelligent birds, especially, parrots. There is considerable sensitiveness to temperature changes. Taste, and probably smell, appear to be deficient.[2] The "sense of support" is marked, even in the young.[3]

Since the pigeon seems to dream and also to miss its absent mate, some observers believe that imagery is present. There is certainly local memory, and also capacity to observe. Various intelligent acts

[1] D. Ferrier: The Functions of the Brain, p. 111, London, 1886.

[2] A. Hill: Can Birds Smell? Nature, vol. 71, pp. 318, 319, 1905.

[3] W. Mills: The Nature and Development of Animal Intelligence, pp. 248, 250, New York, 1898.

have been reported.[1] The remarkable homing habits of the carrier pigeon have received no satisfactory explanation. While Cyon [2] suggests the stimulation of the nasal organs by air currents, Thauziès [3] holds to the electrical theory; they agree, however, that certain higher psychical processes are probably involved.

Graber's [4] tests indicate that pigeons have no color-preference. Beebe's [5] statement concerning birds in general is peculiarly true of pigeons: "There are few species which do not show the emotions of love and sympathy, and . . . one will sometimes pine and die of grief at the loss of its mate." After referring to their patient care of the young, he adds: "Indeed, sympathy is the keynote in the development of the higher mental faculties." These birds communicate, but their language consists of comparatively few sounds. As in many other birds, the play-instinct is highly developed.

II. Problems and Methods

My study of the pigeon's *emotional life* had for its object certain respiratory "expressions." These were investigated by means of a pneumographic tracing, secured while the animal was comfortably fastened in a shallow nest, partially open below. A small box was placed over the bird, and apparatus was so arranged that the time of giving various stimuli was recorded automatically on the smoked paper of the kymograph drum, below the breathing-curve. A third line indicated rate of drum movement. Although some interesting results were obtained, the chief significance of the research consists in its demonstration of the fact that this method of studying animal mind is valuable.

In the study of *association* I sought to determine the sense-data which the process involves, its method of formation (with due regard to social conditions), its rapidity, permanence, and modifiability, and also its probable degree of complexity. Material contributing to the subject was secured by observing the behavior of the animal

[1] Hachet-Souplet: Examen Psychologique des Animaux, pp. 33–38, Paris, 1900. See also Riverside Natural History, vol. 4, pp. 240, 241, Cambridge, 1888.

[2] Orientation chez le pigeon-voyageur, Revue Scientifique, vol. 13, pp. 352–359, 1900.

[3] Orientation du pigeon-voyageur, Revue Scientifique, vol. 2, pp. 417–420, 453–457, 1904.

[4] Grundlinien zur Erforschung des Helligkeits- und Farbensinnes der Tiere, p. 102, Prague, 1888.

[5] Some Notes on the Psychology of Birds, Seventh Annual Report of the New York Zoölogical Society, p. 154, 1902.

when seeking to obtain food by overcoming such obstacles as labyrinths with wire passages, and latches, when the food was left in view, or by finding it when out of sight. In the latter case it was placed in a box occupying a customary place in a group of exactly similar boxes, or else in a box of color or form unlike the other members of the group and variously arranged, from time to time, with respect to them. When the animals were learning the labyrinth habits, various stimulations were given them; later the character of some of these was altered, and the resulting changes in behavior were noted. After the habits had been thoroughly learned, the birds were given a rest for some weeks, and then tested again under the old conditions. A few trials were arranged with special reference to the study of imitation; the animals here were tested as to their ability to execute simple but unfamiliar acts, after having only seen them performed by an animal previously trained. Throughout the associational tests the animals very seldom received food in their cages; but as they were tested daily and allowed to satisfy their hunger completely at the last test, they were never in a state of "utter hunger" — a condition which most experimenters think best to avoid. A series of tests, given at the conclusion of the investigation, indicated that the odor of the food had not assisted the animals in reaching it.

In the two series of experiments (emotion and association) thirty-five animals in all were used. They were confined in large cages in a fairly well lighted and ventilated room, and were fed wheat, cracked corn, and occasionally fruit, and kept well supplied with fresh water and sand. They generally remained in a healthy condition throughout the tests, especially during the winter. To exclude, as far as possible, the disturbing influence of fear, they were usually handled only after the room had been darkened. As the noise made by the curtains was objectionable, the birds were tested with the room illuminated by incandescent lamps; the light was turned off before the birds were placed in position for the trials, and again before they were removed from the apparatus to the cages. It is generally agreed that an experimenter should be out of sight when giving a test. I am convinced that it is important to avoid being seen by the animals at any time. This involves great inconvenience, especially when one employs the pneumographic method, but better results are thus obtained.

For practical suggestions as to apparatus and methods I am greatly indebted to Dr. Robert MacDougall, at the beginning of my investigation, and to Dr. Robert M. Yerkes, thoughout. I also owe much

to the researches of Zoneff and Meumann,[1] Thorndike,[2] Mills,[3] Small,[4] and Kinnaman.[5] Porter's [6] interesting study of sparrows was made almost simultaneously with the investigation here reported. Fewer animals were used by him, but in some instances more tests were given.

III. Investigation of Emotion [7]

1. *Respiration in general.* The normal breathing-curve in pigeons is quite similar in contour to that of the human subject, although the rhythm is more rapid and the pauses are less pronounced. When acoustical, visual, olfactory, or tactual stimuli are given, various modifications appear, for example, quickening, deepening, and minor irregularities. It was noticed that meaningless stimuli (pistol-shots) quickly lose their disturbing influence, whereas the breathing remains sensitive to those of a significant character, such as the noises made by other birds. It was also found that a stimulus which no longer affects the breathing will sometimes occasion disturbance if accompanied by a second stimulus of another order, although of a weak intensity (summation).

2. *Respiratory reactions to light.* As the easy control of conditions makes vision an excellent field in which to work, light reactions were investigated in detail. Two distinct series of tests were given. One sought to determine the relation between quality of light and reaction; the other, between intensity of light and reaction. Four colors of one intensity and three intensities of one color, respectively, were used. In the first series four stimuli, one for each of the colors, red, yellow, green, and blue, were given daily; in the second series five daily stimuli were given, of the same intensity for any one day, and one minute

[1] Ueber die Begleiterscheinungen psychischer Vorgänge in Athem und Puls, Philosophische Studien, vol. 18, pp. 7–14, 1901.

[2] Animal Intelligence, pp. 8–12, 31–36, 51–55, New York, 1898.

[3] Nature of Animal Intelligence and Methods of Investigating It, Psychological Review, vol. 10, pp. 262–274, 1897.

[4] An Experimental Study of the Mental Processes of the Rat, American Journal of Psychology, vol. 11, pp. 135–164; vol. 12, pp. 206–210, 1900–1901.

[5] Mental Life of Rhesus Monkeys in Captivity, American Journal of Psychology, vol. 13, pp. 97–148, 180–210, 1902.

[6] A Preliminary Study of the Psychology of the English Sparrow, American Journal of Psychology, vol. 15, pp. 313–346, 1904.

[7] For a more complete report of this special part III, see the writer's paper, Respiration and Emotion in Pigeons, Journal of Comparative Neurology and Psychology, vol. 15, pp. 494–513, 1905.

apart; this made it possible to observe also the effect of repetition. Each stimulus was given at the beginning of a respiration and continued two seconds. When the tracings were studied, various modifications were noted, but special attention was paid to alterations in rate of breathing. In the case of both sets of trials an immediate quickening usually occurred after stimulation, and occasionally shallowing [1] and minor irregularities of contour.

In the first set of tests ten animals were used for twenty-five days. Average results indicated that red and yellow are less stimulating than green and blue. To secure data that would assist in the interpretation of these results, an investigation was made of the animals' color-preference. This was done by recording, at thirty-minute intervals, the position of the birds when confined, singly, in a box one half of which was illuminated (from the side) by light of one color, and one half by light of different color but of the same intensity. A water screen excluded the heat rays. After nine records had been taken the colored glasses were interchanged, and the animal's position relatively to the two colors was observed as before. This was repeated with the other colors until each of the four had been used with each of the other three. There were far more choices of green and blue than of red and yellow, though none of the colors was avoided. It seemed a question of *degree of liking*, rather than of liking or disliking. As stated, Graber's experiments indicated that pigeons have no color-preference, but his results are probably untrustworthy, since he tested several animals at once and apparently was not careful to change the colored glasses regularly. Putting together our two sets of data (the latter stated first) we have the following comparison:

	R	Y	G	B
Color choices of 5 animals	72	129	167	172
Breathing-rise of 10 animals	9.94 %	10.39 %	10.41%	12.11 %

Although the proportions do not hold, there is a direct correspondence between the two series of responses; hence it would seem that *increased respiratory activity is an expression of agreeable feeling* in pigeons, and this especially since the breathing, when varying at all

[1] If shallowing accompanies quickening, the respiratory activity may be no greater than before; but since depth alterations were seldom observed in these trials after the first day of experimentation, the rise in rate may be taken as a fair measure of the influence of the stimulus.

in amplitude, usually became shallower, and also showed certain minor irregularities of contour, as often occurs in human respiration during moderate stimulation of a pleasant character.[1]

In the second series of respiratory tests four animals were used for fifteen days. Average results showed nothing as to the relation between intensity of stimulus and amount of quickening, since the three intensities used, 1, 2, and 4, produced reactions, respectively, as follows: 6.6 %, 4.3 %, and 6.4 %. This may have been because the three intensities were employed each on different days. When the reactions are averaged according to daily succession, without regard to the intensities of the stimuli, we get the following results: first reaction, 8.0 % rise in rate; second, 3.7 %; third, 4.1 %; fourth, 5.7 %; and fifth, 6.9 %. We should have expected the second daily response to be less vigorous than the first, since the animals were perhaps better prepared for the second stimulation. That the reactions increased thereafter was probably due, partially to summation, and partially to the fact that the short illuminations occasioned mental action (*perception* of interior of box, increased *desire* to escape, etc.) which involved heightened, rather than depressed, breathing activity, and thus worked directly against the dulling tendency of repetition.[2]

IV. Investigation of Association

1. *Labyrinth experiments.* Four labyrinths in all were used (L, M, H, O). Each was constructed by attaching moveable wire partitions in a wooden box, covered with chicken wire. The pigeon was admitted through a small entrance compartment which was fastened at one end of the box, and which communicated with it by means of a lifting door, operated by pulling a cord from behind the observation curtain. Food was placed within the maze, and usually at the opposite end. Before beginning the tests the bird was allowed to become thoroughly familiar with the box without the partitions. After a few trials it learned to go to the food immediately upon entering the box. The partitions were then put into position, and the bird was tested as to the time required (except in the case of labyrinth O) and as to the method employed in reaching the food. The time was measured by means of a stop-watch, and the bird's horizontal movements were recorded on a small plot of the labyrinth; other general observations were added.

[1] P. Zoneff und E. Meumann: *op. cit.*, pp. 57, 58.

[2] R. MacDougall: The Physical Characteristics of Attention, Psychological Review, vol. 3, pp. 162, 176, 177, 1896.

A. *Habits in Labyrinth L*

In this labyrinth (Fig. 1) six animals were tested once daily for thirty days, and five of these again after two and six weeks, respectively. On entering the labyrinth with the partitions in place the first time, a bird started on its usual direct course toward the food-box; running against the first partition it made vigorous efforts to push through, flying at the wire and often clinging to it for a short time;

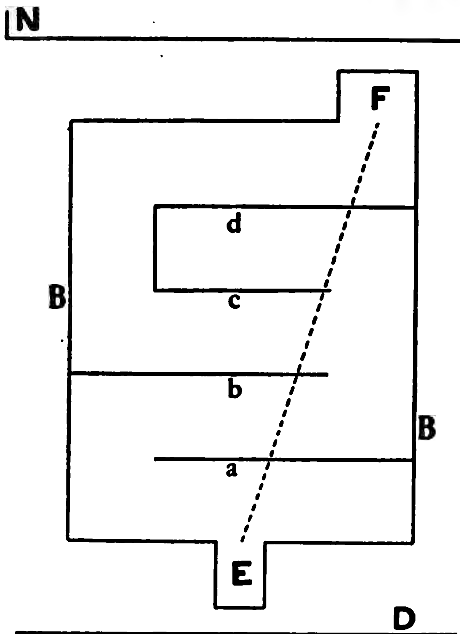


FIG. 1. Labyrinth L. BB, box 6 in. high; E, entrance; F, feeding-place; a, b, c, and d, moveable partitions 6 in. apart; NN, edge of pigeon cage; D, observation screen. The lamps which illuminated the room hung directly above the apparatus.

some of these random movements eventually brought it to the left of the compartment, and thence, through the opening, into the second compartment, and so on through the others, until finally it reached the food by a series of fortunate accidents.[1] The same general reaction was shown in case of the next few tests, except that fewer and fewer useless movements were made, and that the right ones were carried out with greater and greater precision. Later the animal had no

[1] Thorndike: *op. cit.*, pp. 13–15.

difficulty in reaching the food; it did not run against the partitions, enter the blind alley, nor display such general signs of uncertainty as pausing and looking about. The process of learning in this case was obviously one of "trial and error," or the selection of useful movements. From the mass of random movements constituting the reaction to the unfamiliar environment, only those which enabled the bird to reach the food were retained and improved; the others gradually disappeared until finally the path taken became the shortest one possible, and was entered upon and pursued without hesitation by each animal as soon as it was allowed to enter the labyrinth. The time required for the tests is given in Table I.

It will be seen at a glance that the absolute time required for reaching the food varied for the individuals (see especially the results given by different birds in the case of test 1), but that the several periods for any one bird were relatively similar to those for another; and also that the time was long at first, but rapidly shortened from test to test, thus showing a steady advance in the learning process. Various lapses occurred (for example, A, 8; C, 13; E, 10) after the habit had been fairly well fixed.

In tests 18–22 the time-shortening was due principally to quickening of movements which had already become well defined. The great importance of visual data is brought out by the abrupt lengthening of the periods in the case of tests 23–25, and 26–30, where the light intensities were decreased. The lengthening was roughly proportional to the change of illumination. In the relative darkness the birds had to reacquire the habits. The same mistakes were made as at first (running against partitions, and into the blind alley), yet here, as before, there was a ready adjustment. That the food was out of sight, or at least very much less visible, probably made no difference, since it was found that the birds would readily go to the old place after both food and food-box had been removed. In order to exclude the light entirely without making their movements invisible to me, I blindfolded the birds by means of a thin black hood, comfortably adjusted over their eyes and top of head; as a result, none was able to make the course in twenty minutes. The first turn, however, was usually made naturally, perhaps because associated with certain non-visual sense-data (sound of the lifting door, and perhaps tactual impressions of the close entrance compartment, etc.). Rats [1] seem far less dependent upon visual data than do pigeons. The great permanence of the pigeons'

[1] Small: *op. cit.*, vol. 12, pp. 236, 237.

TABLE I. TIME REQUIRED TO REACH FOOD IN LABYRINTH L

	Trials, 1 daily.	A	B	C	D	E	F	*Average*
		Animals						
		′ ″	′ ″	′ ″	′ ″	′ ″	′ ″	′ ″
	1)	28:50	:59	42:20	49:04	22:13	4:04	24:35
	2)	7:22	:22	25:47	10:17	:48	2:02	7:46
	3)	1:18	:12	8:29	12:35	:12	1:41	4:05
	4)	:32	:21	10:51	1:26	:19	:52	2:24
	5)	:24	:28	2:36	2:18	:12	1:33	1:15
	6)	:25	:26	1:10	:55	:12	1:50	:50
	7)	:15	:24	:28	:32	:15	2:09	:41
	8)	1:05	:23	:33	1:19	:17	1:46	:54
[1]	9)	:16	:24	:57	:58	:10	:26	:32
	10)	:24	:32	1:15	:51	2:12	:31	:58
	11)	:12	:21	1:40	:30	:17	:54	:39
	12)	:16	:32	:49	1:34	:22	1:18	:49
	13)	:13	:18	2:30	:18	:10	:42	:42
	14)	:29	:32	:27	:31	:25	:36	:30
	15)	1:00	:24	:30	:31	:12	:35	:32
	16)	:19	:52	1:10	:22	:17	:24	:34
	17)	:14	:14	:31	:39	:13	:57	:28
	18)	:13	:09	:29	:13	:16	:29	:18
	19)	:10	:10	:36	:26	:07	:14	:17
[2]	20)	:11	:15	:34	:17	:07	:10	:16
	21)	:13	:14	:34	:16	:09	:21	:18
	22)	:09	:16	:26	:14	:08	:11	:14
	23)	:12	:42	1:29	:39	5:53	:13	1:31
[3]	24)	:20	:17	1:31	:33	:15	:13	:31
	25)	:15	:24	:40	:28	:19	:20	:24
	26)	1:21	16:29	1:22	13:54	3:51	1:36	6:26
	27)	3:36	4:45	:44	1:03	1:09	2:59	2:23
[4]	28)	:51	1:24	:46	1:04	:56	1:09	1:02
	29)	:51	:41	1:10	:40	2:17	:11	:58
	30)	2:04	:18	:41	:14	:07	:19	:37
	[5] 31)	:08	:33	:25		:07	:07	:16
	[6] 32)	:09	:15	:20		:08	:31	:17

[1] With 18-candle-power illumination of the room.

[2] Same illumination; tests given after the animals had heard four other pigeons pecking in the labyrinth.

[3] With 2-candle-power illumination, other conditions the same.

[4] With a slight illumination through single curtain, other conditions the same.

[5] After two weeks' rest, conditions as in 2.

[6] After six weeks' rest, conditions as before.

habits is shown by comparing the periods for tests 31–3, given after two and six weeks of rest, respectively, with those for tests 18–22.

Let us now notice the gradual progress of learning in three important parts of the maze, as shown in Fig. 2. It will be seen that in the beginning the animals started upon their usual course and pressed against the first partition (stage 1), but that later they touched it less and less (stages 2 and 3), and that finally they avoided it entirely (stage 4). The adjustment here was fairly simple: the sound made by the opening of the entrance door, and the glimpse thus given of the labyrinth, gradually came to be conditions of the movements of turning to the left, on emerging from the entrance, and passing along the compartment toward the opening, where impressions, mostly visual, in the same manner determined the movements of turning to the right and entering compartment 2.

The blind alley was naturally a decided obstacle. The pigeons learned to avoid this compartment by going around it only after many unsuccessful attempts to go through it. During the first test the animals entered it many times (stage 1); on emerging they returned to the second or the first compartment, only to encounter the pen again when they re-advanced toward the food; finally, on reëmerging from the annoying enclosure, perhaps for the eighth or tenth time, they might happen to turn to the right instead of going forward as usual toward the entrance of the box, and thus make their way along the new passage and reach the food. For the next few tests they usually entered the blind alley, but less frequently, and they remained for shorter periods (stages 2–4). Later they merely entered (stage 5); and still later they passed very near the opening without entering, or only paused a moment before it (stage 6); and finally they passed it without the slightest hesitation, walking briskly, but with well-directed movements, midway between the partitions (stages 7–8). The act of turning seemed to be an especially important factor in this habit. We notice that it was a turn to the right (most probably accidental) which first enabled the animals to get beyond the opening of the blind alley; that this same act was repeated in each successive trial until, by the gradual shortening of the loop forming the path taken by the animals in passing into, and from, the labyrinth it was finally reduced to a mere pause (stage 6); and that this later disappeared entirely, leaving only the left turn, which instead was now conditioned by the visual data at that part of the labyrinth and carried the animal past the entrance of the blind alley.

The animals did not come in contact with the second partition until

they had almost learned to pass the opening of the blind alley (see stage 6). This was probably because the turn to the left which was made

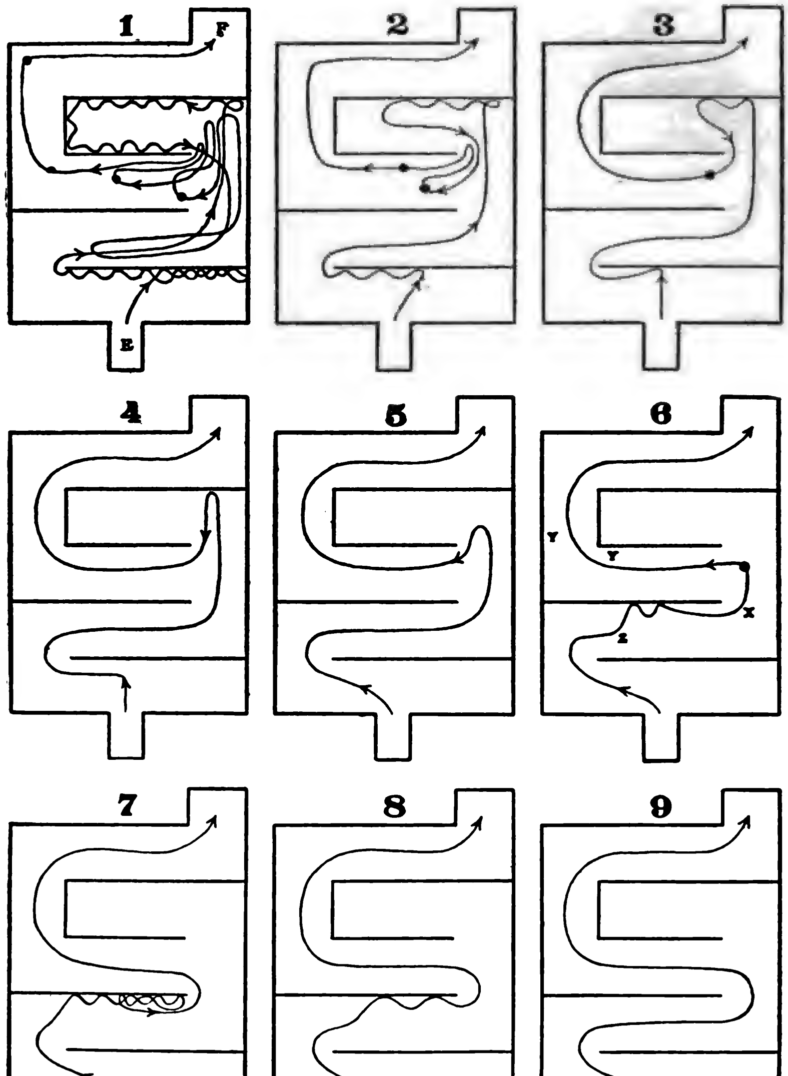


FIG. 2. Stages in learning Labyrinth L. The curves indicate the pigeon's horizontal movements. Pauses are represented by heavy dots.

on approaching x was associated with visual data derived from points farther along the course (y), and when the animals reached z, compartment 2, these same data were received and were sufficient to occasion the turn to the left there also, thus bringing the birds against the partition. The adjustment was made principally on the basis of new sense-data arising from running against the wire, looking at it more closely, etc. For a few trials the birds made the turn at x too quickly, and thus failed to reach the third compartment. One of my most intelligent subjects made this mistake in the third test, and again in the sixth and seventh, and retained the act almost unchanged through the tenth, eleventh, twelfth, thirteenth, fourteenth, sixteenth, seventeenth, twentieth, and twenty-first tests, so strong was the tendency to continue a movement once begun, though it was really disadvantageous.[1]

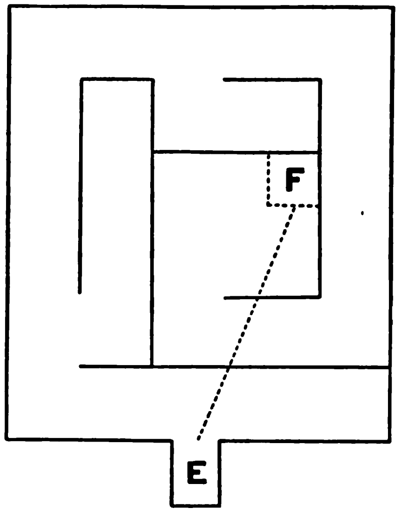


FIG. 3. Labyrinth M. E, entrance; F, food-box; height of large box and width of passages the same as in Labyrinth L.

After reaching the food and satisfying their hunger, the animals often returned to the maze passages, seeking an exit; but they never "explored" passages or showed other evidence of "free curiosity" and "desire to know all their new surroundings," as Small reports concerning rats.[2]

B. *Habits in Labyrinth M*

Five of the animals previously used were next tested twice daily, forenoon and afternoon, for five days, in a larger, more complicated

[1] Small (*op. cit.*, vol. 11, p. 146) states that in his rats "the persistence of useless motor habits is striking" and "explainable by the supposition that the movements are touched off automatically."

[2] *Op. cit.*, vol. 12, p. 214.

maze. It had two blind alleys, and the food-box was near the centre (see Fig. 3). The animals' general behavior was similar to that before observed. The periods are given in Table II.

TABLE II. TIME REQUIRED TO REACH FOOD IN LABYRINTH M

	Animals					
Trials,	A	B	C	E	F	*Average*
2 daily.	′ ″	′ ″	′ ″	′ ″	′ ″	′ ″
(1)	16:25	2:55	6:33	3:26	4:11	6:42
(2)	:55	4:10	2:24	3:36	:23	2:18
(3)	1:12	:55	8:27	9:06	2:07	4:21
(4)	:48	:44	2:31	:34	1:04	1:08
(5)	:27	:16	:14	:16	:14	:17
(6)	:18	:32	:25	:15	:27	:23
(7)	:14	:11	:31	:44	:30	:26
(8)	:12	:16	:57	:16	:48	:30
(9)	:12	:18	:23	:16	:16	:17
(10)	:10	:19	:16	:15	:28	:18

Although this maze was much more difficult, it will be seen that the animals learned the route to the food far more readily than before. The first period in this series was only about one fourth as long as the first period in the other, and the course was mastered sooner (by the fifth trial instead of by the ninth). There was less pressing against the wire than before, and unsuccessful movements were sooner discontinued. This improvement was probably due entirely to experience gained in dealing with the first maze. Thorndike speaks of the gradually increasing ability of animals to deal with successive contrivances.[1] The average results given in Tables I and II are plotted in Fig. 4, next page.

C. *Habits in Labyrinth H*

Since hearing is an important sense in pigeons, we should expect them to be capable of useful acoustical associations. Several things occurred in the course of the two preceding experiments which seemed to indicate that this is true; for example, although I could move about, rather noisily, in the darkened room, without apparently disturbing any of the birds, some few showed signs of fright (moving about restlessly) on hearing the low, grating noises made by lifting the hanging door of the cage, sounds which had always preceded the handling of the subjects before experimentation, and which had probably become signs to them of being taken.

[1] *Op. cit.*, p. 28.

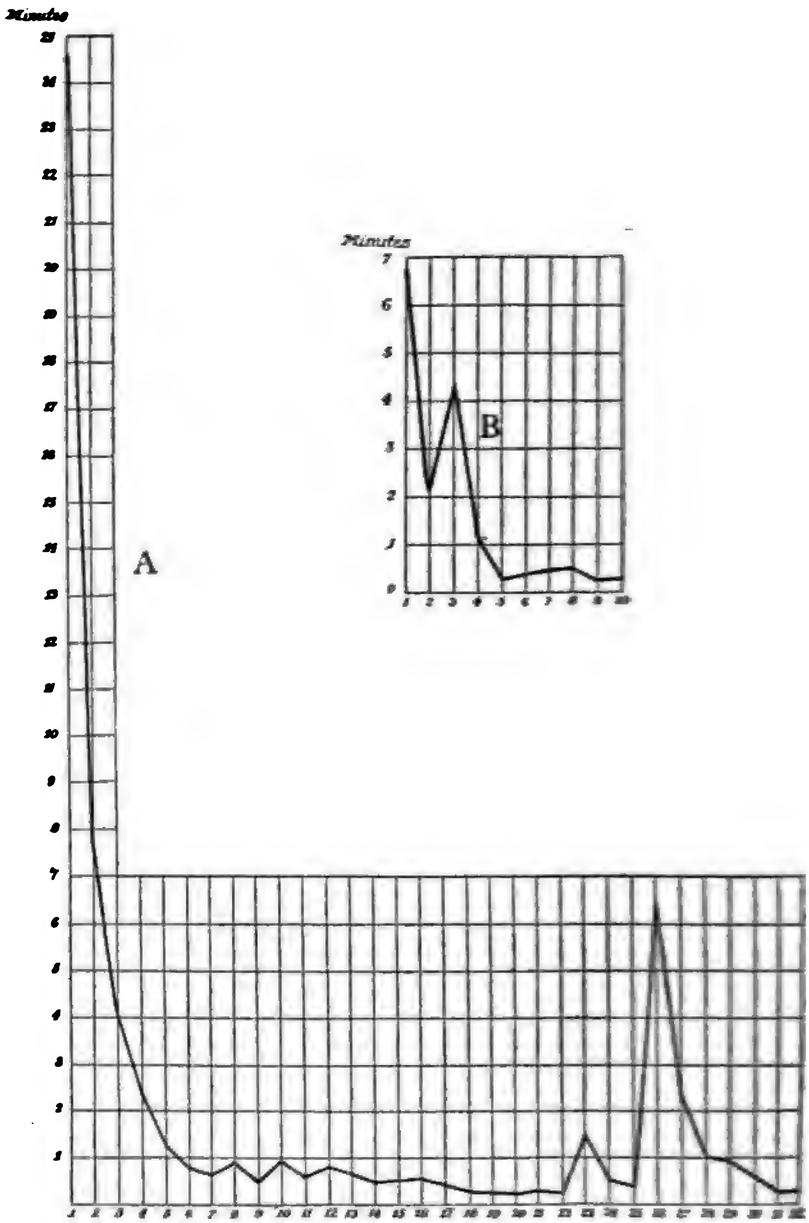


FIG. 4. Learning-curves: A, in Labyrinth L; B, in Labyrinth M. Divisions of ordinates indicate minutes; of abscissas, successive daily trials. The rise curve A for tests 23 and 26 was due to diminution of illumination of room. a

To investigate this kind of association I constructed a labyrinth (see Fig. 5) in connection with which sounds could be utilized as one form of sense-data. The passages were so arranged that along the route leading to the food there were three blind alleys, which the animals would surely enter before mastering the course. In another part of the room were placed, very close to each other, two electric gongs of the same size, but of different material. One was of metal and gave a clear ringing sound; the other was of wood and gave a low rattling noise.

When an animal was learning the route (as in the other two mazes) I sounded the gongs, the metallic, as the bird approached and entered the blind alleys, by openings *M*, *O*, and *R*, and the wooden, as it emerged from them and proceeded along the proper course, and occasionally after it had reached the food. The ringing sound was also given after the animal passed *P* and was approaching *Q*. When the new route was fairly well learned, I changed the order of the sound stimuli, ringing one gong at the places where the other had previously been sounded, and compared the records thus obtained with those obtained when the sounds were given in the original order. As an animal is liable to become confused by the sounds, or else quickly accustomed to them, I thought it best to give only a few trials, one trial with the usual order of sound stimulations, the next immediately following with the reversed order, and so on till four pairs of records had been secured, the series of trials being completed in a single day. Four animals were thus tested. The periods of the various trials are shown in Table III.

TABLE III. SOUND ASSOCIATION, LABYRINTH H

Time required to reach food under different sound conditions

I

Order of gongs the same as when course was being learned.

Trials	A	B	E	G	
	"	"	"	"	
(1)	13	19	24	17	
(2)	16	12	13	16	
(3)	10	16	20	14	
(4)	12	19	10	12	
Total,	51	66	67	59	" 243

II

Order of gongs reversed.

Trials	A	B	E	G	
	"	"	"	"	
(1)	14	16	18	27	
(2)	37	17	16	27	
(3)	14	19	26	11	
(4)	27	22	21	14	
Total,	92	74	81	79	" 326

In the case of thirteen of the sixteen tests g
acoustical conditions (see column II) the period
the corresponding ones given alternately with th
and there was an average time-lengthening of 5.2
34.2 %. The following is a short description of th
to the changed conditions. It corresponds to the ti
in column II of the table.

Bird A: test 1, animal undisturbed; test 2,
turned to the left and went toward *R*, but later r
S without pausing; test 3, paused at *O* for a sh
not enter the blind alley; test 4, paused at *O* aga
from *S*, turned to the left and entered blind alley
and this time passed *S* without being disturbed,
at *T* and *U*.

Bird B: tests 1 and 2, animal apparently undist
slight pauses at openings; test 4, drew back fro
alley 3, but soon escaped and passed *S* without h

Bird E: test 1, undisturbed; tests 2 and 4, p
test 3, turned back from *S*, entered blind alley 3,
eral places later when passing toward F.

Bird G: test 1, many pauses; test 2, turned
blind alley 3; test 3, undisturbed; test 4, drew
toward *R*, but did not enter the blind alley.

As the animals gave little attention to the woo
always sensitive to the metallic one, their observe
ably must be accounted for chiefly on the basis o
organic sense-data ***now with***, and ***now without***,
The data governing the start (as already noticed)
ficient for the avoidance of the first blind alley w
reversed. In case of the other two, however, t
to depend upon acoustical data, and when these
approached the openings *O* and *Q*, the left turn
be initiated, hence certain hesitations and mis
ment frequently occurred. Experience with the
first experiment assisted the animals in dealing w
alley here, but mistakes were made. Visual data
cient to produce the proper turn at *Q*, but when
was given just afterwards, it sometimes occasione
bringing the animals toward the opening of the
the tests given were not such as would indicate l
discriminate sounds, they certainly show that the

of useful sound associations, although visual ones are evidently of greater importance to them.

D. *Habits in Labyrinth O*

I next made tests in which tactual and electrical sense-data could also be utilized. In one of the passages of a simple labyrinth was placed a board 8 in. square and $\frac{3}{4}$ in. thick, over which were stretched copper wires which formed a series of interrupted electrical circuits. By closing a key a bird could be stimulated whenever it stepped upon the wire surface. A second key was connected with a metallic gong. When an animal on its way through the maze first stepped upon the wire surface, electrical and acoustical stimuli were given; later it was allowed to walk across the board without being thus stimulated; afterward acoustical stimuli were given it at various parts of the maze.

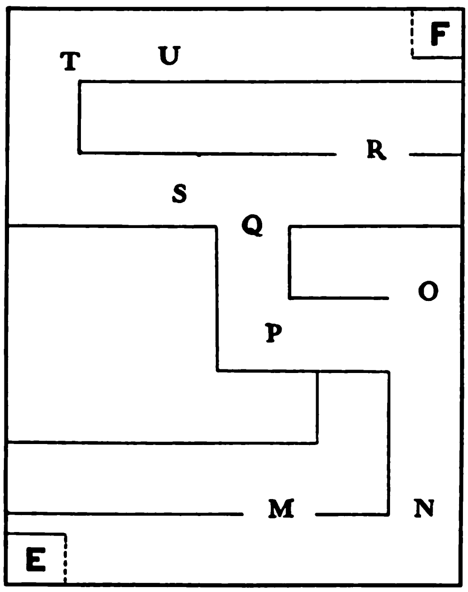


FIG. 5. Labyrinth H. For study of acoustical association. Passages 6 in. wide, as before.

Eight animals were used. All were found quite sensitive to the electrical shocks, and when next tested they avoided the board, especially if the gong sounded as they approached. Some would show signs of uneasiness anywhere in the maze on hearing the gong. When the board was so placed that they had to pass over it in reaching the food, *when once on it* they moved very leisurely, often lingering; and if they stepped upon the wire surface in the darkened maze, they showed no evidence of being frightened. Evidently no association had been formed between the peculiar tactual stimulus of touching the wires and the electrical shocks which had at first been given. But the tactual stimulus may have been below the threshold. Yerkes[1] saw evidence of association of this kind in the frog; this animal, however,

[1] The Instincts, Habits and Reactions of the Frog, Harvard Psychological Studies, vol. 1, pp. 591–593, 1903.

is probably much more sensitive to tactual stimulation received from surfaces over which it passes than is the pigeon.

The results of these four experiments indicate that the pigeon easily acquires complicated labyrinth habits; that these remain fixed for some weeks at least; that acoustical, visual, and certain organic data are the most important sensory factors; and that the process of learning is one of "trial and error," in which the animal comes to form such a close connection between the sense-data of the interior of the box and those other sense-data arising from movements involved in reaching the food, that when the box impressions are again encountered the other sense-data are revived and readily condition the proper movements. How much memory of eating was involved in these tests cannot be told; but it was certainly not an essential part of the mental act.[1] Proper guidance throughout the course was the main thing, and this was determined by definite sense-data. That recognition, discrimination, and perhaps choice were to some extent present seems likely from the animal's hesitating movements at certain critical points. Thus it is highly probable that when the bird approached a blind alley which it had always entered before (see Fig. 2, stage 6), two alternatives were recognized, to enter, as before, or not to do so, as was usual thereafter, and that the pause had for its mental correlate a state closely bordering upon what in us would be deliberation.

2. *Release experiments.* Under this heading are included certain cage experiments in which some act, such as touching a lever, pecking, or stepping upon a platform, resulted in the opening of the door, and thus enabled the animal to escape and secure the food lying in view without. The animal was admitted to the cage through an entrance compartment as in the case of the maze trials. Before being tested it was allowed to become familiar with the cage and to reach the food directly by passing out through the open door. When first in the cage the animals did not seem to notice the release apparatus, and hence they probably did not begin learning the method of escape until later when they entered the cage and found the door closed, and the ordinary exit thus obstructed.

A. *Latch Tests*

The cage here employed was an 18-inch cubical box. The top was of chicken wire and the bottom and three sides of heavy boards; the fourth side was formed by narrow vertical bars and a wire door

[1] Small: *op. cit.*, vol. 12, pp. 230, 231.

which opened inwardly and was held by a latch working on the outer surface of the bars. At first a long wooden latch was used, which the animals raised when reaching out for food. As this seemed an unnatural act, downward pressure was substituted by attaching to the latch, now made smaller and of brass, a string which ran over a pulley above the door and down into the cage. As nooses did not seem adapted to the birds, the end of the string was attached to a wooden lever which worked on the inner surface of the bars, about three inches from the floor. Eight animals were tested four times daily (twice in the forenoon and twice in the afternoon) for ten days. The time required to escape and the animals' behavior were recorded as in the case of the labyrinth tests.

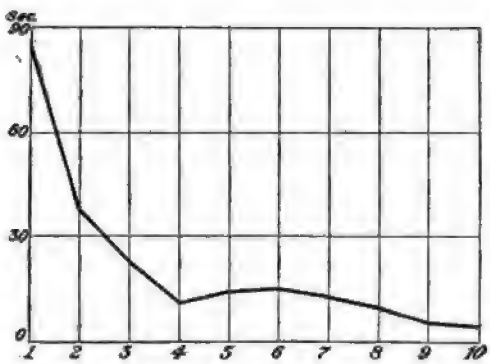


FIG. 6. Curve of learning to operate latch, plotted from last column of Table IV. Each vertical division indicates 30 sec. The horizontal divisions represent successive days.

When they first entered the box (singly as in the other experiments) and found the usual exit closed they made various attempts to push through between the bars, springing and often flying about with great force and persistency. In course of their random movements they touched the bar and opened the door and thus escaped. Later the unnecessary movements were mostly dropped and the necessary ones became highly specialized. The first association was established between the box impressions received on entering and the movements involved in approaching the front of the box and depressing the lever; later a connection was formed between the sensations of touching the lever, of hearing the sound of the opening door, of feeling the jar, etc., and the movements of turning away from the lever and passing out. The sight of the opening door seemed to be of less service to the birds than the sound and jar. Each animal soon came to touch the bar at the point of least resistance, and usually with considerable precision. The time required by the several birds is shown in Table IV, next page. The daily average results are plotted in Fig. 6, above..

TABLE IV. TIME REQUIRED TO ESCAPE FROM CAGE BY USING LATCH

Trials, 4 daily.	*Animals* A	B	C	E	F	G	H	I	Av.	Daily Average
	′ ″	′ ″	′ ″	′ ″	′ ″	′ ″	′ ″	′ ″	′ ″	′ ″
(1)	:03	:06	3:20	:08	:03	1:10	4:30	:23	1:13	
(2)	:05	1:10	:59	1:50	2:00	:10	2:46	1:00	1:00	
(3)	5:28	:29	1:04	:21	:29	:52	2:33	4:05	1:55	
(4)	:31	1:45	3:52	:14	:06	:23	2:40	2:30	1:30	(1:25)
(5)	:15	:10	2:25	:03	:10	:07	:31	:50	:34	
(6)	:17	2:00	5:03	:03	:05	:15	:39	:10	1:04	
(7)	:08	:20	:36	:31	:03	:11	:30	:46	:23	
(8)	:03	:54	:47	:28	:03	:55	:06	:47	:30	(:38)
(9)	1:12	:34	:39	:17	:03	:04	:21	:20	:26	
(10)	:02	:19	:28	:07	:01	:03	:20	:47	:16	
(11)	:02	:51	:09	:14	:03	:02	1:46	:47	:29	
(12)	:02	:15	:24	:12	:02	:02	1:17	:27	:20	(:23)
(13)	:03	:19	:22	:07	:06	:04	:48	:11	:15	
(14)	:02	:15	:22	:09	:02	:02	:20	:19	:11	
(15)	:08	:06	:12	:05	:02	:02	:03	:06	:06	
(16)	:03	:18	:44	:10	:02	:03	:02	:21	:13	(:11)
(17)	:02	:17	:41	:03	:07	:07	:11	:06	:12	
(18)	:02	:42	:26	:05	:01	:03	:31	:15	:16	
(19)	:04	:13	:48	:04	:01	:02	:15	:14	:13	
(20)	:04	:32	:35	:04	:01	:02	:08	:22	:15	(:14)
(21)	:03	:13	:10	:13	:09	:01	1:10	:16	:17	
(22)	:03	:05	:10	:11	:03	:02	:39	:26	:12	
(23)	:02	:07	:17	:05	:03	:15	:20	:26	:12	
(24)	:02	:14	:04	:02	:01	:02	:09	1:03	:12	(:12)
(25)	:01	:03	:05	:04	:01	:02	:31	:24	:09	
(26)	:02	:04	:08	:01	:01	:05	1:34	:33	:19	
(27)	:02	:03	:03	:03	:10	:05	:15	:39	:10	
(28)	:02	:04	:03	:01	:06	:05	:23	:51	:12	(:12)
(29)	:02	:04	:10	:03	:02	:06	:23	:21	:09	
(30)	:01	:03	:03	:02	:11	:02	:09	1:08	:12	
(31)	:21	:03	:11	:03	:01	:03	:28	:11	:10	
(32)	:03	:02	:04	:02	:01	:03	:11	:42	:09	(:10)
(33)	:02	:02	:03	:10	:01	:03	:18	:17	:07	
(34)	:02	:02	:03	:02	:01	:03	:11	:11	:04	
(35)	:01	:02	:03	:02	:01	:02	:18	:09	:05	
(36)	:01	:03	:03	:03	:03	:02	:04	:10	:04	(:05)
(37)	:01	:02	:07	:02	:04	:01	:11	:09	:05	
(38)	:01	:02	:02	:02	:02	:01	:08	:03	:03	
(39)	:02	:04	:11	:01	:02	:02	:14	:08	:06	
(40)	:01	:02	:03	:01	:01	:03	:10	:03	:03	(:04)

The periods here were similar to those given in the maze tests — the time was long at first, then it shortened very rapidly for a few trials, then more slowly but still constantly, until the act became thoroughly familiar. The process was one of "trial and error" throughout. As before, various lapses occurred, even although the animals were as persistent as usual in their efforts to escape. When the lever was moved to the side or back of the box, none of the animals could escape. In general, pigeons show less ingenuity in dealing with latches than do sparrows, according to Porter's [1] observations, although in some other tests they are equally apt.

B. *Pecking Tests*

The preceding series of trials proved the animals' ability to utilize certain touching or clawing movements, at first accidental, in making an escape, and showed that these could become highly specialized. Desiring to carry out similar tests in the case of pecking movements, which are quite as natural to pigeons, I arranged a contrivance by means of which the act of pecking at corn-grains fastened to a small piece of cardboard (placed just outside the cage, but within easy reach through an opening in the wire) would open the cage-door by making a delicate electrical contact. Four animals were tested.

On entering the cage they endeavored to escape as before; failing in this they began pecking about until they found the corn-grains and made the contact which opened the door and allowed them to escape to the food without. Three of the animals made their escape in this way several times; but the habit seemed to be one that could not be readily learned, as the successive periods showed little shortening.

Thinking that the pecking of things at a definite place perhaps complicated the matter, I removed the electrical apparatus and arranged to open the door myself by pulling a string whenever the pigeon pecked anywhere upon entering the box. Preliminary experiments with Bird J indicated some ability to profit by this kind of experience. As the act of pecking could be used to advantage in a series of imitation trials, this animal only was allowed to learn to escape by actually pecking; the others were reserved for the imitation tests next to be reported.

[illegible] *Imitation Test*[illegible]

[illegible] the experiment[illegible] reported the anima[illegible] were used [illegible] ally and usually out of sigh[illegible] the others, although in the [illegible] room and within hearing [illegible] them. [illegible]hen ef[illegible]rts were made [illegible] some of th[illegible]

experiments to test the animals in a separate room, signs of fear and discontent were often noticed, and it was necessary to return to the first room to continue the tests. Some instances were noticed in which a pigeon would do what it saw another doing. For example, one of my subjects would not eat one day, being ill apparently; but when I put two others into the compartment with it, and they began eating the food lying about, it also began pecking. Its act could not have been due to its only then happening to see the corn, for it had before looked toward the food when this was thrown to it.

Desiring to test, under definite conditions, the imitative ability of these animals, I arranged trials in which birds were allowed to see a useful, but simple act performed by another bird, and then were given an opportunity to execute the act themselves. Using the animal which I had trained for the purpose, I allowed its series of acts (entrance to box, pecking, release, and food-eating without) to be observed by another animal, confined in a small wire compartment (similar to the entrance compartment before used) attached to the side of the large cage. Care was taken to see that the confined bird was observing, or at least was looking toward the acting one; in case of doubt, the trial was repeated. Later the trained animal was replaced by the observing one, and the latter's reaction was noted. Five animals, in all, were tested, and each was given two opportunities to escape after having seen the trained animal perform the act ten successive times. None of them, however, showed any signs of trying to escape by repeating the movements so often performed by the bird familiar with the act, but each rushed against the sides of the cage and tried to push through at various places, just as the trained bird had done when first learning the habit.

As the act, or series of acts, was rather too complex to be easily observed and utilized by the other pigeons, I arranged two much simpler tests. In one case the leading bird was taught to open the cage-door by stepping upon a platform (the lowering of which made an electrical contact); in the other, to avoid a blind alley, enter a short passage, and ascend a wooden plane (inclined at an angle of thirty degrees) which led into another box containing food. In these tests it was more difficult for the series of acts to be viewed, but the animals, singly as before, were placed at a point of vantage and apparently saw the movements of the other animals.

Of the five birds tested in the platform experiment, four utterly failed to escape in the two trials given. The fifth, in its second test, went to the platform promptly and thus made its escape, but the suc-

cess may have been accidental, or due to the animal's experience of seeing and approaching the platform in its first test. In the labyrinth experiment only one bird (second test) avoided the blind alley and went directly up the inclined plane to the food; this success was probably due to experience in the first test. There was certainly no evidence that the animals had grasped the nature of the problem; nothing to indicate that the performance of the trained animal had supplied data for the guidance of future conduct, that is, for the conditioning of the necessary movements, in this case those of pecking, stepping upon a platform, or avoiding a blind alley and ascending an inclined plane.[1]

These results are similar to those which other experimenters have secured in the case of chicks, cats, dogs, monkeys,[2] and also rats.[3] Although the method I employed is doubtless open to the criticism of being artificial,[4] some value at least should be attached to the results; if so it would seem probable that imitation in pigeons is not above the "instinctive" stage, and that learning depends entirely upon first hand experience, upon really doing the thing, and not upon merely seeing it done.

3. *Position, Color and Form Tests.* The apparatus used in these experiments consisted of small boxes, six inches in height, and open at the top. Sometimes they were exactly similar, and sometimes they differed in color or in form. They were moveably attached, six inches apart, to a board which was placed in a large wire-covered box, having an entrance compartment as in the case of the mazes and cages. Food was placed in one of the small boxes, and the pigeons were allowed to find it twice; later each bird was tested as to its ability to return to the food by depending upon the position of the box in the group, or upon its color or its form. Tests were given in series of six, and the box which was first approached was recorded as the animal's choice for that test. If it made a wrong selection, it was allowed to look about until it found the box containing the food, but in no case was it permitted to satisfy its hunger until the last test of the series. The animal apparently did not see the food until it approached the box, and subsequent tests demonstrated that it was not guided by odor.

[1] C. L. Morgan: Animal Behaviour, pp. 179–193, London, 1900; Animal Life, and Intelligence, p. 453, Boston, 1891.

[2] Thorndike: *op. cit.*, pp. 54, 56, 57, 60, 61; The Mental Life of the Monkeys, Psychological Review, Monograph Supplement, vol. 3, pp. 318, 319, 1901. Kinnaman: *op. cit.*, pp. 198–200.

[3] Small: *op. cit.*, vol. 11, p. 160.

[4] W. Mills: Nature of Animal Intelligence and Methods of Investigating It, Psychological Review, vol. 10, pp. 262–274, 1897.

A. *Position Tests*

In this series of trials I used at first six, later nine food-boxes, four inches square and covered with dark gray paper. The board to which the several receptacles were fastened was shifted at irregular intervals to various oblique angles; this was done to prevent the animal from being assisted by the position of the food-box in the larger box rather than in the group of similar boxes. After a bird had been tested sufficiently for one position it was then used, for a week or so, in some other experiment, and thus given an opportunity to forget, to some extent at least, the old experience before being taught to find the food in a box placed elsewhere in the group. For the positions 2, 3, and 4, in the group of six similar boxes, eight animals were each given thirty tests in series of six, as stated above. For the positions 5, 6, and 7, in the group of nine similar boxes, the experiments were shortened. Six animals were given twenty-four trials each, two animals for each of the three positions.

The animals quickly learned the position of the food-box and passed to it promptly when released from the entrance compartment. Changing the position of the board to which the food-boxes were attached did not affect the animals' ability to reach the food readily. They usually selected the proper box as before, although frequently they went around the end of the board and approached the food from the opposite side. The general distribution of choices in the case of positions 2, 3, and 4 is given in Table V; the rate of learning, in Table VI.

With a single exception (Bird B, box 3) the box containing the food was far more often chosen than any one of the empty boxes, and usually more often than all of them combined, the average right choices being: position 2, 62 %; position 3, 57.7 %; position 4, 53.3 %. It will be seen that the animals were more successful in finding the food in the second position than in either the third or the fourth, that is, positions nearer the end were more easily located. But what is of greater interest to us is the rate of learning to go to the right box. This is indicated by the increasing number of right choices from series to series. (Table VI.) That the increase was small was due to the fact that the animals learned so quickly in the first series of six tests that little improvement could be made thereafter; what they could learn they acquired early in the experiment. There is some evidence of improvement after the first series in the case of box 4, a more difficult position, and the average for all three boxes shows a slight improvement from series 1 to series 5, although a falling-off is seen in the last series: 26, 27, 27,

31, 28. The same general features appear in the case of the incomplete tests (positions 5, 6, and 7). For each box there were, on the average, 26 right choices in the possible 48. There were more right choices in the case of box 7 than in the case of either box 6 or box 5, box 7 being nearer one end (box 9). There was little evidence of learning after the first series of trials.

TABLE V. ASSOCIATION OF POSITION: GENERAL DISTRIBUTION OF CHOICES

Choices of boxes 1 to 6 when food was placed in boxes 2, 3, and 4

	Food in box 2 *Boxes*						*Food in box 3* *Boxes*						*Food in box 4* *Boxes*					
Animals	1,	2,	3,	4,	5,	6.	1,	2,	3,	4,	5,	6.	1,	2,	3,	4,	5,	6.
(B)	1	18	4	6	1	0	1	1	8	7	8	5	0	0	2	21	4	3
(C)	2	20	6	1	1	0	0	9	19	2	0	0	0	7	2	15	6	0
(E)	4	18	2	3	2	1	0	6	18	3	3	0	2	6	2	15	3	2
(F)	5	20	1	2	2	0	1	6	18	3	2	0	0	2	5	14	8	1
(G)	0	18	5	2	2	0	0	3	13	5	7	2	0	0	3	17	9	1
(H)	0	22	4	3	1	0	1	0	18	4	6	1	0	4	8	15	3	0
(I)	4	18	3	1	3	1	0	1	23	6	0	0	0	6	7	15	2	0
(J)	1	17	7	5	0	0	3	0	21	3	2	1	1	4	3	16	6	0
Total,	17	151	32	26	12	2	6	26	138	33	28	9	3	29	32	128	41	7

TABLE VI. ASSOCIATION OF POSITION: DISTRIBUTION OF RIGHT CHOICES

Choices from series 1 to series 5 in the case of boxes 2, 3, and 4

	Box 2 *Series*					*Box 3* *Series*					*Box 4*[1] *Series*				
Animals	1,	2,	3,	4,	5.	1,	2,	3,	4,	5.	1,	2,	3,	4,	5.
(B)	4	4	5	2	3	2	2	1	1	2	4	4	4	5	4
(C)	5	4	2	5	4	4	4	4	4	3	3	3	2	2	5
(E)	3	4	4	3	4	4	4	2	5	3	3	2	2	4	4
(F)	5	4	3	5	3	5	3	3	4	3	0	2	4	4	4
(G)	3	4	4	3	4	2	2	4	2	3	3	3	2	4	5
(H)	4	4	4	5	4	4	3	4	5	2	3	3	4	3	2
(I)	1	4	3	5	5	4	4	5	6	4	2	1	3	4	5
(J)	4	5	4	3	1	3	4	6	4	4	3	3	3	4	[illegible]
Total,	29	33	29	31	29	28	26	29	31	24	21	21	24	[illegible]	[illegible]

Average for the three boxes, 26, 27, 27, 31, 28.

The method of learning in these position test[illegible] was the same [illegible] noticed i[illegible] previo[illegible] experiment[illegible] namely build[illegible] [illegible] [illegible]nance [illegible]

cesses. When first admitted to the large box containing the row of small ones at the farther end, the animal accidentally found the receptacle containing the food, and later associated the movements involved in reaching that position with various sense-impressions of the box, especially those experienced upon entering — certain tactual impressions of the small entrance compartment, sound of the lifting door and sight given of the interior of the large box.

While the results clearly indicate that pigeons readily learn the position of objects, nothing is proved as to "counting." Some experimenters speak of similar trials as "number-tests," just as they do of "form-tests," but this is probably going too far. To investigate counting in animals, experiments should be arranged which mimimize spatial responses. These tests certainly show that pigeons can discriminate positions readily, especially toward the ends of the group, but little more is certainly indicated. Porter [1] says: "If we do not find in birds the power to count, we have in their nice sense for the location of a member of a series . . . something of that preliminary number-sense which Ribot describes as belonging to children and savages."

B. *Color Tests*

To investigate the animals' ability to utilize colors [2] in finding their food, I employed the same apparatus as before, except that six boxes were used throughout and each was covered with paper of a different color: red, yellow, green, blue (Bradley's standards, except red, *RO* being substituted), gray, and black. The boxes covered with black and gray paper were employed merely to complete the group of six. The same method as before was employed, except that the board to which the boxes were attached was left stationary at the end of the large box, and also that the position of all six boxes was changed irregularly for each test.

The general behavior of the animals at the beginning of these tests was quite similar to that shown in the preceding experiment; but it was soon evident that colors occasioned them far more difficulty than positions. The general distribution of choices is given in Table VII. It will be seen that the proper box was usually chosen more often than any one of the empty ones, but never oftener than the other five combined, as occurred in the position tests; also that in the case of each

[1] *Op. cit.*, p. 335. See also C. L. Morgan: Introduction to Comparative Psychology, p. 232, London, 1900.

[2] Cornish states (Animals at Work and Play, p. 30) that hunters near the Caspian are able to decoy partridges by use of brilliant colors.

color there were instances in which another color was as often, or more often selected. Yet it is clear that colors may serve as valuable sense-data for these animals. In the first series of six tests (see Table VIII) there were few right choices or none, but in each succeeding series the number increased. The learning process was evidently of the same type as before observed (selection, in this case gradual, of chance but useful movements), and involved visual data largely.

Table VII. Color Association. General Distribution of Choices

Choices of all 6 boxes when food was placed in red, yellow, green, or blue boxes

	Food in Red Box						*Food in Yellow Box*						*Food in Green Box*						*Food in Blue Box*					
Animals	R,	Y,	G,	B,	B'k	G'y.	R,	Y,	G,	B,	B'k,	G'y.	R,	Y,	G,	B,	B'k,	G'y.	R,	Y,	G,	B,	B'k,	G'y
(B)	12	6	7	3	2	0	2	12	6	7	0	3	4	5	12	4	5	0	1	3	5	10	6	5
(C)	15	8	1	4	2	0	3	12	1	6	3	5	3	4	14	5	1	3	3	1	2	13	2	9
(E)	11	8	5	3	3	0	3	10	5	3	5	4	1	9	7	11	0	2	3	5	5	10	3	4
(F)	7	5	9	6	2	1	6	6	5	6	3	4	0	8	9	6	0	7	0	3	6	11	5	5
(G)	9	1	7	5	3	5	5	11	6	3	2	3	2	5	16	2	4	1	5	5	4	5	4	7
(H)	13	3	5	3	3	3	5	9	6	3	3	4	1	3	15	5	6	0	7	5	5	6	4	3
(I)	11	5	2	4	6	2	4	10	5	4	3	4	0	4	15	6	2	3	5	5	3	9	5	3
(J)	10	0	6	8	4	2	5	10	3	3	5	4	4	1	12	9	1	3	6	4	4	7	5	4
Total,	88	36	42	36	25	13	33	80	37	35	24	31	15	39	100	48	19	19	30	31	34	71	34	40

Table VIII. Color Association. Distribution of Right Choices

Choices from series 1 to series 5 in the case of red, yellow, green, and blue boxes

	Red Box					*Yellow Box*					*Green Box*					*Blue Box*				
Animals	1,	2,	3,	4,	5,	1,	2,	3,	4,	5,	1,	2,	3,	4,	5,	1,	2,	3,	4,	5,
(B)	0	1	2	4	5	1	2	3	2	4	1	2	2	4	3	1	3	1	2	3
(C)	2	2	4	3	4	1	2	3	3	3	3	2	3	4	2	3	2	2	3	3
(E)	2	1	2	3	3	1	0	3	2	4	0	1	1	1	4	0	1	3	2	4
(F)	0	1	1	2	3	0	2	2	1	1	0	1	2	3	3	1	1	2	4	3
(G)	0	2	3	2	2	2	2	1	3	3	2	4	3	3	4	1	1	0	0	3
(H)	1	1	3	4	4	0	2	2	2	3	2	3	4	3	3	0	1	2	2	1
(I)	1	2	3	3	2	1	2	2	3	2	1	3	3	3	5	0	2	2	3	2
(J)	1	2	2	2	3	1	2	1	2	4	1	1	3	3	4	1	2	1	1	2
Total,	7	12	20	23	26	7	14	17	18	24	10	17	21	24	28	7	13	13	17	21

There is no evidence that the color-preference of the animals assisted them in choosing correctly; in fact, they were rather less successful in dealing with those colors for which they had previously shown decided preference,[1] since the whole number of right choices

[1] See the writer's paper, Respiration and Emotion in Pigeons, *op. cit.*, p. 502.

was less in the case of the green and blue boxes (85) than in the case of the red and yellow ones (92), and since there was a relative diminution in the rate of learning toward the last in case of the former boxes.[1]

To test the animals' ability to discriminate shades of colors in finding their food, two birds were used, with four boxes, each covered with a different shade of red paper, and two with the boxes covered with green paper. The brightness of the different shades was not measured, but to the eye it seemed to be equal in each of the cases. The food was placed in the box having the most nearly saturated color, and twenty-four trials in series of six, as before, were given each bird. The results were quite similar to those secured with different colors. With the red shades there were twenty-two choices of the best saturated shade to eight, ten, and eight, respectively, of the other three; and with green, twenty-one to nine, ten, and eight. The 43 correct choices were distributed from series 1 to series 4 as follows: 7, 11, 12, and 13, which shows learning as before. The relatively large number of right choices was probably due, partially to the fact that fewer alternative choices were possible since only four boxes were used, instead of six, and partially to the fact that the box containing the food may have been slightly brighter than the others.

Throughout these trials the position-element was a decidedly disturbing factor. When the animals were first learning to choose a box of a definite color, some would show a marked tendency to approach a receptacle occupying a certain position, and would persist in this from series to series. Others at first showed no special preference for certain positions, but, after happening to make a correct choice, they would return to that same place the next time, and thus miss the right box which had been changed for the new test.

C. *Form Tests.*

In this experiment the six food-boxes were each of different form: triangular, square, oblong, hexagonal, circular, and elliptical. They were of the same capacity, and were covered with light-brown paper. As in the preceding experiment, the birds were tested for only four of the boxes, and were given thirty trials each. Six animals were used, and as they were not the same as those previously employed, the

[1] One of Porter's sparrows was less successful with yellow and red than with blue and green. He says: "This may be partly explained from the fact that she was more afraid of these." *Op. cit.*, pp. 338, 339. See also E. L. Thorndike: Instinctive Reactions of Young Chicks, Psychological Review, vol. 6, pp. 283–284, 1899.

square box (which had always been used before) had no advantage over the others in attracting the birds at the beginning of the trials. The tests were given as in the preceding experiment, except that it did not seem necessary to change the position of each of the six forms before giving each test; it was thought sufficient to move the food-box, and, if a wrong choice had been made in the preceding test, also the box wrongly chosen. The results are shown in Tables IX and X.

Table IX. Form Association. General Distribution of Choices

Choices of all 6 boxes when food was placed in Tri., Sq., Hex., or Cyl. boxes

	Food in Tri.						*Food in Sq.*						*Food in Hex.*						*Food in Cyl.*					
Animals	Tri.	Sq.	Ob.	Hx.	Cyl.	El.	Tri.	Sq.	Ob.	Hx.	Cyl.	El.	Tri.	Sq.	Ob.	Hx.	Cyl.	El.	Tri.	Sq.	Ob.	Hx.	Cyl.	El.
(U)	8	6	5	5	2	4	5	9	6	1	6	3	5	5	3	11	3	3	6	5	3	5	8	3
(V)	9	2	3	6	7	3	5	8	5	2	5	5	4	4	5	8	5	4	6	6	3	4	7	4
(W)	8	7	3	4	5	3	5	8	5	3	3	6	3	5	3	10	7	2	4	3	4	3	10	6
(X)	11	5	2	5	3	4	5	8	3	6	3	5	4	5	4	7	6	4	2	5	4	4	9	6
(Y)	11	4	5	4	2	4	4	9	5	6	3	3	3	4	2	8	6	7	3	2	5	6	9	5
(Z)	8	3	7	4	4	4	6	11	7	3	1	2	5	4	2	11	3	5	3	5	4	4	9	5
Total,	55	27	25	28	23	22	30	53	31	21	21	24	24	27	19	55	30	25	24	26	23	26	52	29

Table X. Form Association. Distribution of Right Choices

Choices from series 1 to 5 in the case of Tri., Sq., Hex., and Cyl. boxes

	Food in Tri.					*Food in Sq.*					*Food in Hex.*					*Food in Cyl.*				
Animals	1,	2,	3,	4,	5,	1,	2,	3,	4,	5,	1,	2,	3,	4,	5,	1,	2,	3,	4,	5,
(U)	2	0	2	1	3	1	2	2	2	2	2	1	2	3	3	1	1	2	1	3
(V)	1	1	2	3	2	2	1	1	2	2	0	1	2	3	2	1	2	2	1	1
(W)	1	2	1	2	2	2	1	2	1	2	1	2	2	2	3	1	2	2	3	2
(X)	1	2	3	3	2	0	2	2	3	1	1	2	2	1	1	1	1	2	3	2
(Y)	1	2	3	2	3	2	2	2	3	2	1	1	2	1	3	0	2	2	2	3
(Z)	1	2	2	2	1	0	2	3	3	2	1	2	2	4	2	1	2	3	1	2
Total,	7	9	13	13	13	7	10	12	13	11	6	9	12	14	14	5	10	13	11	13

It will be seen that each animal chose the right box oftener than any other one box, but not oftener than all of them; also that there was a small increase in the number of right choices from series to series. No one of the four forms seemed better discriminated than the others if we may judge from the practical equality of right choices made in each case (55, 53, 55, 52) or from the similar increase in number of right choices from series to series; the hexagonal and cylindrical boxes received fewer choices in the first series than did the tri-

angular and square, but this was exactly counterbalanced in the last series. The triangular box was more often confused with hexagonal and square, and the square with triangular and oblong, than with the others. For the hexagonal box the cylindrical was more frequently mistaken than were the other forms, especially the oblong; and with the cylindrical the elliptical was more frequently confused than were the others, especially the oblong. In this series of tests nothing new as regards general behavior or method of learning was observed.

TABLE XI. POSITION, COLOR AND FORM ASSOCIATION

	Total Right Choices[1]	*Right choices from series 1 to series 5*[2] 1	2	3	4	5
Position	57.9 %	54.2 %	55.6 %	56.9 %	63.2 %	59.0 %
Color	35.3 %	16.2 %	29.7 %	37.0 %	42.7 %	51.6 %
Form	29.8 %	17.4 %	26.4 %	34.7 %	35.3 %	35.3 %

If we compare the results obtained in these three experiments (see Table XI and Fig. 7), we shall see that the pigeons were governed much more by the position of the food-box than by either its color or its form, and that color was better associated than form. Position was a most important factor throughout, as was observed also by Porter [3] in the case of the English sparrow. Porter [4] also found that his sparrows could associate color better than form. In the position-tests the pigeons showed very little improvement from series to series (see table); almost all that the animals could learn was acquired at the beginning. The more difficult color- and form-trials, however, showed almost constant improvement, although we should have expected this to be greater in the latter case than it was. When judged entirely by the actual number of right choices in a given kind of tests, some of the birds made a very poor showing; but from the standpoint of increasing number of right choices they appeared in a wholly different light.

[1] Tables V, VII, and IX.
[2] Tables VI, VIII, and X.
[3] *Op. cit.*, p. 338.
[4] From the tables (*op. cit.*, pp. 330–339) it seems that the right choices for position,color, and form, were respectively, 40 %, 58 % and 20 %. The comparatively small number of correct position choices was probably due to his using ten boxes instead of six, as in the other two series. My results given in Table XI were secured under almost exactly comparable conditions. Compare results of Kinnaman in case of the Rhesus monkey, *op. cit.*, pp. 130, 131, 134, 141, and 177.

Thus, for example, bird F (Table VII) made only 33 right choices in a possible 120, yet their arrangement is significant, being, from series 1 to series 5, respectively, 1, 5, 7, 10, 10. It is probable that there would have been still greater improvement had the tests been continued; perhaps the animal would have become as proficient in finding its food by depending upon the color of the receptacle usually containing it, as by relying upon the position of the box in the group.

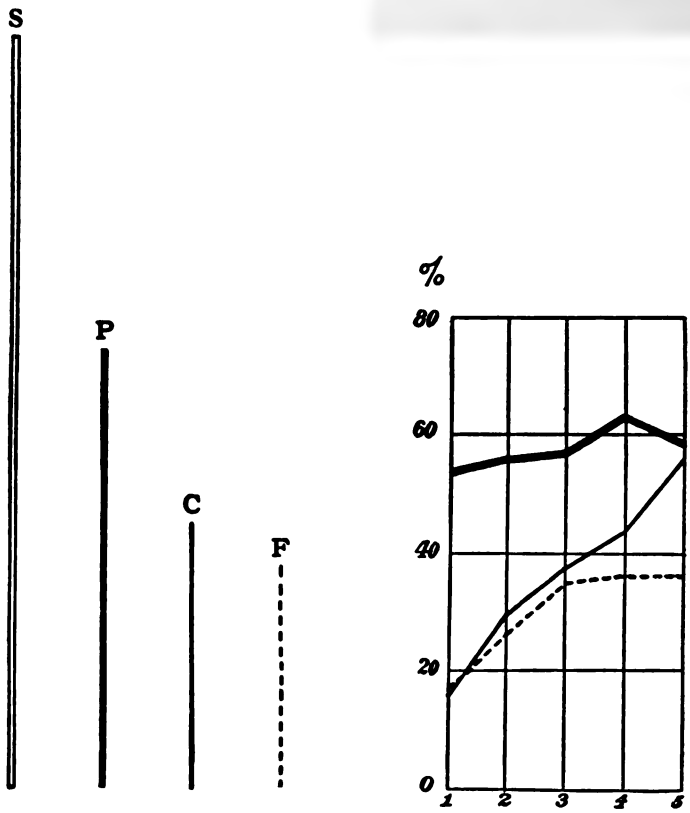


FIG. 7. Position, Color, and Form Association. If line S represent tests of a given kind, P, C, and F would represent, respectively, the number of correct choices of position, color, and form. The rate of learning in each case is shown by the corresponding curves to the right, where vertical divisions each indicate 20 %, and horizontal divisions the successive series in which the tests were given.

V. Summary

1. Respiration in pigeons is sensitive to various stimuli, and since its alterations of rate, amplitude, etc., can be easily recorded pneumographically without frightening the animals, it may well serve as a process through which to study their mental life.

2. By repetition meaningless stimuli, for example, pistol-shots, quickly lose their disturbing influence; whereas the breathing remains sensitive to those of a significant character, such as the noises made by other birds.

3. Reaction to light of moderate intensity consists principally in an immediate quickening, the amount varying with the color; since a direct correspondence was found between color-preference and breathing-rate, it would seem that here agreeable feeling involves increased breathing activity.

4. Visual, acoustical, probably tactual, and certainly organic data, are the principal sensory factors of the associations of pigeons.

5. The animals readily form useful associations by a method of "trial and error," or the selection of successful movements which were at first accidental.

6. Apparently a pigeon does not learn by merely seeing a new act performed by another pigeon; yet there are instances of simple ("instinctive") imitation, and "trial and error" learning is not wholly independent of social conditions, since it proceeds much more satisfactorily if the animal is trained at least within hearing distance of other pigeons.

7. When a habit is being formed, the "period" required for the first test is usually very long, but learning proceeds quite rapidly during the next few trials; later it is more gradual, but it continues till the act becomes thoroughly familiar.

8. Associations are fairly permanent, and some remain practically unaltered for at least six weeks. Modification is easily accomplished, however, on the basis of new experience.

9. Pigeons differ widely both as to the ease with which they acquire associations and also as to their permanence. Difference in activity seems the chief reason for this.

10. While these birds seem mentally inferior to English sparrows and to various mammals which have been tested in a similar manner, they are capable of numerous ready adjustments. They discover circuitous labyrinth passages, they learn to manipulate latch apparatus when adapted to their natural habits and conveniently placed, and

they easily reach their food by depending upon the position, color, or form of the box containing it. But the process is apparently simple association throughout. There is no evidence of higher mental activity — no looking the situation over and acting accordingly, no "reasoning" in the proper sense of the word, but only blind movements, some of which are retained and become highly specialized, merely because successful.

REACTIONS OF THE CRAYFISH

BY J. CARLETON BELL

THE crayfish has long been the typical Crustacean for anatomical and physiological investigations, but it is only recently that its reactions to sensory stimuli have been made the object of experimental study. The purpose of this paper is to describe the reactions of the animal to certain sensory stimuli under experimental conditions, and to estimate the relative importance of these stimuli in the life of the organism.

I. REACTIONS TO VISUAL STIMULI

Huxley[1] states that crayfish avoid direct sunlight, hiding under stones during the day, and becoming active in the evening. On the other hand, they are attracted like moths to fires lighted on the bank at night, and may be scooped out by hand. Abbott,[2] giving an account of the burrowing crayfish, *Cambarus diogenes*, states that it is very difficult to observe the animals at work, since all their digging is done at night. It would seem from the account of Miss Hoppin, quoted by Garman,[3] that the blind crayfish, *Cambarus pellucidus*, is not altogether insensitive to light, for, reporting on the fauna of the caves of Missouri, she says that the crayfish are all found near the entrance to the cave, where there is considerable light. In the dark recesses there are only little white fishes. Blind fish and crayfish are also taken from the wells in the neighborhood, where the crayfish are found only in wells that are rather shallow and light; the fish, on the other hand, are only obtained from deep, dark wells.

According to the above accounts it would appear that the crayfish is negatively phototactic to direct sunlight or diffuse daylight, but

[1] An Introduction to the Study of Zoölogy, illustrated by the Crayfish, Internat. Sci. Ser., 1880.

[2] How the Burrowing Crayfish works, Inland Monthly, Columbus, Ohio, vol. 1, pp. 31, 32, 1885.

[3] Cave Animals from Southwestern Missouri, Bull. Mus. Comp. Zoöl. Harv. Univ., vol. 17, pp. 225–239, 1889.

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[3] Cave Animals from Southwestern Missouri, Bull. Mus. Comp. Zoöl. Harv. Univ., vol. 17, pp. 225–239, 1889.

positively phototactic to a light at night, and moreover, that light may influence the behavior of the animal even when the eyes have ceased to function.

The directive influence of light upon the movements of the crayfish has never been experimentally studied to my knowledge. Dearborn[1] thinks that light has no effect upon the animals. Yerkes[2] and Towle[3] have shown that Daphnia move toward the light. Bethe[4] finds that Carcinus is negatively phototactic, and also shows a tendency to hunt out corners. When the eyes are varnished with lampblack, the phototaxis disappears, but the tendency to seek out corners still remains. Bethe says that he has observed the same phenomenon in the crayfish. Keeble and Gamble[5] discovered that *Hippolyte varians* responds positively to light under all conditions, and Palæmon is just as markedly negative. *Macromysis*, however, reacted now positively now negatively, depending on the background. A black (absorbing) background called forth a positive response, while a white (scattering) background produced a negative reaction. Spaulding,[6] in studying the habits of the Hermit Crab (*Eupagurus*), found that it is strikingly positively phototactic. When animals are placed in an aquarium, one half of which is shaded, none of them are ever noticed inside of the dark line. Herrick[7] notes that lobsters are nocturnal, and avoid the light when placed in a tank, and Bateson[8] says that prawns and shrimps lie hidden during the day, and are active only at night. Parker,[9] in a study of Copepods, finds that the females have a strong positive phototaxis for light of a low

[1] Notes on the Individual Psychophysiology of the Crayfish, Amer. Jour. of Physiol., vol. 3, pp. 404–433, 1900.

[2] Reactions of Entomostraca to Stimulation by Light, II, Reactions of Daphnia and Cypris, Amer. Jour. of Physiol. vol. 4, pp. 405–422, 1900; Reactions of Daphnia pulex to Light and Heat, Mark Anniversary Volume, pp. 359–377, 1903.

[3] Heliotropism of Cypridopsis, Amer. Jour. of Physiol., vol. 3, pp. 345–365, 1900.

[4] Das Nervensystem von Carcinus mænas, I, Arch. f. mikros. Anat., vol. 50, pp. 460–547, 589–640, 1897.

[5] The Color Physiology of Higher Crustacea, Phil. Trans., London, Series B, vol. 196, pp. 295–388, 1904.

[6] An Establishment of Association in Hermit Crabs (*Eupagurus longicarpus*), Jour. Comp. Neur. and Psych., vol. 14, pp. 49–61, 1904.

[7] The American Lobster; A Study of its Habits and Development, Bull. U. S. Fish Comm., vol. 15, pp. 1–252, 1895.

[8] Notes on the Senses and Habits of some Crustacea, Jour. Marine Biol. Assoc'n., Plymouth, N. S., vol. 1, pp. 211–214, 1889.

[9] The Reactions of Copepods to Various Stimuli, Bull. U. S. Fish Comm., vol. 21, pp. 103–123, 1902.

intensity, while males show a weak negative phototaxis. To light of over 100-candle power at a distance of 10 cm. or to direct sunlight the female Copepods are negative, while the reaction of the males does not seem to be altered.

In his work on *Carcinus*, Bethe obtained retraction of the eye-stalks by suddenly throwing a strong light on the eye by means of a mirror. "Usually the eyes were quickly drawn in and protruded again, sometimes several times in rapid succession, like a man blinking under a sudden, strong light." When a dark object, the size of the hand, was moved just over the water, the eyes were seldom retracted, but the antennules were usually drawn in. Lemoine [1] observed that in Astacus retraction was due to touch alone, and that no light, however strong, was able to bring about such a reaction. Gulland [2] takes just the opposite view with reference to Astacus, stating that there are no setæ of any sort on the eye-stalk, and therefore it is insensitive to touch, but is withdrawn only because the animal sees the object by which the stimulus is given. If a curved needle is used, and the stimulus is applied from behind, no retraction follows. Dearborn,[3] however, working with Cambarus, agrees with Lemoine in saying, "Withdrawal of the ophthalmites into their sockets occurs only on contact with some hard object, — not from any light-stimulus of an ordinary sort." I may say in passing that in none of the following experiments on Cambarus was there ever a sign of retraction due to stimulation by light, the retraction always taking place in response to a touch-stimulus.

Lyon,[4] in his study of compensatory movements of the eye-stalks, found that when the eyes were painted with lampblack, the crayfish showed a reduction of about 10% in the compensatory movements when rotated in vertical planes, but the compensation remained the same for rotation about the dorsi-ventral axis. On rotation in the dark the compensatory movement of the eyes was found to be from 5° to 8° less than in the light.

[1] Recherches pour servir à l'histoire des systèmes nerveux, musculaire, et glandulaire de l'écrevisse, Ann. des Sci. Nat., Series 5, vol. 9, pp. 99–280; vol. 10, pp. 5–54, 1868.

[2] The Sense of Touch in Astacus, Proc. Roy. Physiol. Soc., Edinburgh, vol. 9, pp. 151–179, 1886.

[3] *Loc. cit.*

[4] Contribution to the Comparative Physiology of Compensatory Movements, Amer. Jour. of Physiol., vol. 3, pp. 86–114, 1899.

Experimental

In the investigations to be described, 58 crayfish of the species *Cambarus affinis* were made use of, and for identification the animals were marked on the back with white enamel paint, the males receiving the even numbers from 2 to 64, the females the odd numbers from 1 to 51.

1. *Reactions to White Light*

The questions proposed for investigation were, (*a*) How does the crayfish react to diffuse daylight; (*b*) to reflected sunlight; (*c*) to direct sunlight; (*d*) to artificial light of different intensities? (*e*) What is the influence of previous conditions of exposure to light upon the reactions of the animal? (*f*) Do changes of temperature affect the reactions?

A wooden box, 80 cm. long, 25 cm. wide, and 20 cm. high, painted black on the inside, and constructed so as to hold water, was covered with a heavy black cloth to exclude the light from above. The front end of the box was of glass, thus admitting the light from the end. In all the experiments except those with direct sunlight, this glass end was covered with black cardboard in which a hole 10 cm. long and 5 cm. high had been so cut that the light was admitted at the middle of the bottom of the glass. The direct sunlight was admitted through the whole of the glass end. At the rear of the box a piece of black cardboard was so arranged that an aperture was afforded for observing the animals without admitting any appreciable amount of light, and this aperture could be readily closed by a slide when not in use.

The method of experimentation was to place the animal in the box about 20 cm. from the glass end, and observe whether it went toward or away from the source of light. The animals were experimented on in two groups of five each, and one hundred observations were made on the individuals of each group with each intensity of light, that is, twenty observations on each animal. In order to check the influence of the orientation of the animal at the time of exposure to the stimulus, the following four positions for placing the animal were chosen: (1) Head toward the light; (2) Head away from the light; (3) At right angles to the light with right side toward it; (4) At right angles with the left side toward the light. Thus five observations were made on each animal of each group in each position, exposed to each of the different intensities of light.

Seven different intensities of light were employed, and the results have been arranged in eight sets, as follows: I. Diffuse daylight in dry box, *i. e.*, the animals were taken out of their ordinary medium, water, and were exposed to the stimulus of diffuse daylight in the air. The reactions under these conditions, however, were so slow and so unsatisfactory that the test was abandoned after the first group, and thus the second group has nothing to show for itself under this head. The remaining seven sets of observations were made on animals placed in 10 cm. of water at 15° C. II. Diffuse daylight. III. Reflected sunlight. The box was placed near a window on a clear day, and the sunlight was thrown in horizontally by means of a mirror. IV. Direct sunlight. On a clear day the box was placed in such a position that the sun shone in directly and illuminated the front half of it. V. 9-candle-power incandescent electric light. This lamp was marked 16 c., but it had been used a great deal, and on being tested with a Lummer-Brodhun photometer showed only 9 c. VI. An incandescent electric light of about 50 c. This lamp was marked 100 c., but had been used considerably and was slightly smoked. Unfortunately it was broken before there was any opportunity to test it. Judging from the fact that another 100 c. lamp of the same manufacture, in slightly better condition, measured 64 c., the estimate of 50 c. seemed a safe one. VII. The incandescent electric light alluded to above, which measured 64 c. VIII. An arc light which varied in intensity from 150 c. to 250 c.

In intensities V and VI the lamp was placed 5 cm. from the glass end of the box to allow the interposition of a heat-screen consisting of an alum solution in a flat glass jar 5 cm. thick. Reckoned in candle-metres, therefore, the intensity of the illumination at the surface of the animal in V was 144 c. m., and that in VI was about 800 c. m. In VII two heat-screens were used, and between these was placed a lens of considerable but not accurately determined curvature, so that it is impossible to express the intensity in candle-metres. In VIII the light was so variable that such an expression would mean nothing.

Unfortunately it was impossible to keep the two groups constant throughout the whole series, owing to the death of two individuals in each group during the experimentation. Group 1 was composed of nos. 1, 3, 4, 8, and 9, of which 1 and 8 were replaced by nos. 13 and 42 respectively. Group 2 was begun with nos. 23, 27, 32, 34, and 38, and the vacancies caused by the death of 23 and 32 were filled by nos. 21 and 36. The following table exhibits the reactions

to the different intensities of light, + indicating an orientation toward the source of light, − an orientation away from the light, and ± an indifferent orientation, which usually means no movement at all.

Table I. Summary of Reactions to White Light

	Group 1			Group 2			Totals		
	+	−	±	+	−	±	+	−	±
I.	23	48	29				23	48	29
II.	49	47	4	19	81		68	128	4
III.	40	60		30	70		70	130	
IV.	47	52	1	36	64		83	116	1
V.	51	49		30	70		81	119	
VI.	39	61		39	61		78	122	
VII.	28	72		28	71	1	56	143	1
VIII.	35	63	2	37	60	3	72	123	5
	312	452	36	219	477	4 =	531	929	40 = 1500

The following table gives the average time required for orientation for each group. The time of each animal in seconds was noted with a stop-watch from the instant the animal was placed in the box until a definite orientation was assumed with reference to the light. If no orientation followed within three minutes, the result was called indifferent.

Table II. Average Time of Orientation

	Intensities								
	I	II	III	IV	V	VI	VII	VIII	Ave.
Group 1	(144)	52	11	4½	9	8½	11	19	16½
Group 2		9	3	4	4	5	4	5	5

Inspection of these tables shows that when the animals were taken from the water and placed in diffuse daylight in the air (I), their movements were so sluggish that in twenty-nine cases out of one hundred there was no orientation within the three-minute limit. Moreover, in the seventy-one cases where there was definite orientation the average time was over two minutes (144 seconds). While, therefore, the conditions were quite different from the normal environment of the animal, it is interesting to note that of the cases where orientation did take place the negative reactions were more than twice as many as the positive. In II, where the animals were under the same conditions of diffuse daylight but in the water, a wide variation in the reactions of the two groups is noted. In group 1 they are about equally divided between positive and negative, while in

group 2 there is the largest proportion of negative reactions in the whole series. It will be observed that the time for group 1 is extremely long compared with the other averages, and this doubtless indicates a general sluggishness and lack of sensitiveness to stimuli in the animals, which might to some extent account for the difference in reaction. If we consider the totals of both groups for each intensity, we are led to conclude that there is no appreciable difference in the reactions of crayfish to diffuse daylight, to sunlight, or to artificial light within the limits here employed. A slight exception to this is found in VII, where the 64 c. lamp with the lens caused a somewhat more uniform negative reaction. The action of direct sunlight in IV is rather remarkable in that with the lowest proportion of negative reactions in the whole series we also observe the shortest time-average, indicating that the animals are the liveliest and most sensitive. This would seem to indicate that while the animals are in general somewhat negatively phototactic to all light-stimuli of moderate intensity the action of direct sunlight tends to reduce the negative phototaxis to a minimum. If we consider the totals of the two groups separately we observe that group 1 has only 57 % of negative reactions while group 2 shows 68 %. This is rather in accordance with what we would expect from the general time-average, which is over three times as much for group 1 as for group 2. But although we may in a general way connect rapidity of orientation with a large percentage of negative reactions, it will not do to carry it to individual cases, for it was observed that no. 27 showed 83 % of its reactions negative, yet its total time-average was 10 sec., the highest in its group.

In general, then, we conclude that the crayfish is negatively phototactic in the proportion of about two to one. This apparently contradicts the statement made by Huxley that crayfish "are attracted like moths to fires lighted on the bank at night." For surely if this were the case some such tendency would have been observed in these experiments. On the other hand, there is no such marked and definite response to light as in the case of Daphnia or Hippolyte or Palæmon or the Hermit Crab. The action of the stimulus is by no means mechanical and constant, but there is wide variation in individuals.

As was mentioned in the description of the method of experimentation, four different positions for placing the animal were chosen with the idea that the initial position of the animal with respect to the light might have some influence on the direction of its movement. To determine what this influence might be, a careful record was kept of the orientation with reference to each one of these positions

and the following table gives a summary of these observations. In the table position I is where the animal is placed with its head toward the light; position II, with head away from the light; position III, at right angles to the light with the right side toward it; position IV, at right angles with left side to light.

TABLE III. INFLUENCE OF POSITION ON LIGHT REACTIONS

	Position I			Position II			Position III			Position IV			Totals		
	+	−	±	+	−	±	+	−	±	+	−	±	+	−	±
Group 1	85	108	7	61	135	4	74	114	12	92	95	13	312	452	36
Group 2	39	133	3	48	126	1	57	118		75	100		219	477	4
	124	241	10	109	261	5	131	232	12	167	195	13	531	929	40

Since the animals have been shown to be somewhat negatively phototactic, we should expect that position II, with the head away from the light, would show the largest number of negative reactions, and this is what we find if we take the sum of both groups. But by the same course of reasoning we should expect position I, with head toward the light, to yield the smallest number of negative reactions, a condition which prevails neither in the sum nor in either of the groups. On the whole we can only say that difference of position seems to have remarkably little influence on the orientation of the animals.

In his work on the eye of the crayfish, Parker[1] called attention to the migration of the pigment in the retinular cells under the influence of light. The question now arose, what influence, if any, does this pigment migration exert upon the reactions of the crayfish to light? The time required for pigment migration in the eye of the crayfish has never been determined to my knowledge, but from the work of Parker[2] on Palæmonetes it was thought that confinement in the dark for about an hour would be sufficient to bring about a retraction of the pigment. Accordingly, group 1 was kept in the dark for one hour, group 2 for one hour and a half, before experimentation. A further test was made to observe the effects of pigment expansion, both groups having been exposed for one hour and a half to the rays of a 32 c. incandescent electric light at a distance of 40 cm. The apparatus used in the reaction-tests was the box described above, with the 64 c. light as a stimulus. As to the method of observation,

[1] The Retina and Optic Ganglia in Decapods, especially in Astacus fluviatilis Mitth. Zool. Stat. Neapel., vol. 12, pp. 1–73, 1895.

[2] Photomechanical Changes in the Retinal Pigment Cells of Palæmonetes, etc., Bull. Mus. Comp. Zoöl. Harv. Univ., vol. 30, pp. 273–300, 1897.

each group was placed in the centre of the box at right angles to the horizontal rays of light, and the position of each animal was accurately noted at intervals of one minute for one hour. In reporting the results, all the observations of animals in the half of the box nearest the light are denominated positive, those in the half farthest from the light negative. The results are given in Table IV, where line I indicates the reactions after confinement in the dark, line II those after exposure to the light.

TABLE IV. INFLUENCE OF PREVIOUS CONDITIONS UPON REACTIONS TO LIGHT

Group 1	Nos. 13		3		4		9		42		Totals	
	+	−	+	−	+	−	+	−	+	−	+	−
I	54	6	60		5	55	48	12		60	167	133
II	59	1	60			60	9	51	55	5	183	117

Group 2	Nos. 21		27		31		36		38		Totals	
	+	−	+	−	+	−	+	−	+	−	+	−
I	5	55	5	55	7	53	10	50	4	56	31	269
II	38	22		60	13	47	45	15	16	44	116	184

Let it be said at once that these results do not offer an altogether satisfactory basis for an answer to the question proposed above. Some of the animals would take up a position during the first ten minutes and remain in it for the rest of the hour. Whether the position taken was due solely to the light, or was owing to thigmotactic influences, or whether it depended on the way in which the animal was released, are questions which cannot be answered, and for this reason the conclusions to be drawn from the table are tentative. If we examine the table we find that the totals of both groups agree in manifesting a decrease in negative results for line II, after exposure to the light, as compared with line I, after confinement in the dark. This is what we would expect from negatively phototactic animals. When taken from the dark the pigment is retracted, and the sensitive retinal substance is exposed to the direct action of a rather strong light. The negative tendency of the animal we should expect to find accentuated. The decrease in negative reactions is especially marked with group 2, which was shown above to be much the livelier of the two, and all the individuals except no. 27 share in the change. In group I the decrease is not so striking, and is observed to be due to two individuals solely. Inexplicable is the preponderance of positive over negative reactions in the results for group 1.

All of the experiments thus far described were carried out in water at approximately 15° C. The question naturally arises, what will be the result of raising or lowering the temperature upon the reactions of the animals to light? Unfortunately the experiments anent this question are fragmentary and incomplete, but the results will be given for what they are worth. The same apparatus and the same intensity of light (64 c.) were used as in the preceding paragraph. The results, presented in Table V, are arranged in three sets, as follows: The line marked I represents the results obtained from group 1 at a temperature of 5° C. The animals were placed in the box one at a time, as in Table I, and their orientation noted. They were set at right angles to the rays of light, five times with the right and five with the left side toward the source of the stimulus. No observations were made upon group 2 at 5° C., and those on group 1 are so few as to have a questionable value. Line II gives the reactions of both groups of animals in water at 25° C., and in this set the animals were placed in all four of the positions indicated for Table III. Line III presents the reactions of the animals in water at 25° C. by the method outlined for Table IV, *i. e.*, each group of animals was placed in the centre of the box, and observed at intervals of one minute. To obviate the objection of the animals remaining in one spot, they were reset every ten minutes in the middle of the box, at right angles to the entering rays.

TABLE V. REACTIONS TO LIGHT AT DIFFERENT TEMPERATURES

Group 1	Nos. 13		33		4		42		9		Totals	
	+	−	+	−	+	−	+	−	+	−	+	−
I	7	3	4	6	2	8	2	8	8	2	23	27
II	16	4	14	6	11	9	9	11	13	4(±3)	63	34(±3)
III	19	41	13	47	39	21	17	43	27	33	115	185

Group 2	Nos. 21		27		31		36		38		Totals	
	+	−	+	−	+	−	+	−	+	−	+	−
II	12	8	5	15	12	8	15	5	13	7	57	43
III	12	48	12	48	31	29	42	18	44	16	141	159

Judging from lines II and III we may say that there is a tendency toward a decrease of the negative phototaxis with an increase in temperature. It is true that group 1 in line III maintains the average, 62 % negative reactions, but the others are much lower than this, line II even going over to positive phototaxis in both groups. In line III the animals of both groups were extremely active during the first ten minutes, rushing about from one end of the box to the other, pushing each other back and forth, and in general exhibiting great

restlessness. Some of the animals when first put into the water reacted with a sort of cramp reflex, which was followed in a few seconds by intense activity. After the first ten minutes the animals began to grow more quiet, and in twenty or thirty minutes they had become quite sluggish, scarcely moving out of the position in which they were reset. During the period of restlessness the males showed marked sexual activity, rushing up to the females, pushing them about, seizing them, and trying to turn them over in spite of their vigorous resistance. One of the males, no. 36, did succeed in turning a female on her back twice, although she struggled violently to escape, — a thing which the female never does in the ordinary sexual act. The rise in temperature, therefore, seemed to stimulate the males to sexual activity, but not the females.

2. *Reactions to Colored Light*

No observations have ever been made, so far as I know, on the reactions of the crayfish to colored light. Lyon, in his work on compensatory eye-movements, found that rotation in blue light gave a compensatory movement only slightly less than that in white light, while in red light the compensation was only a little larger than in darkness. In some animals the interposition of an opaque object between the eye and the source of light caused an elevation of the eye 1° or 2° toward the vertical. Red glass acted like an opaque object, blue glass produced no effect, *i. e.*, blue light had the same effect as white light. To observe whether the same thing applied to movement reactions was the object of the following experiments.

a. Reactions to Horizontal Colored Light. The same apparatus was used as in the previous experiments, viz., the dark box with light from the 64 c. lamp entering horizontally at the end. Across half of this end were placed pieces of colored glass of a saturated blue, green, yellow, and red. The colored light obtained by this means was not spectrally pure, but it was the nearest to it that could be obtained. A more serious objection is that the intensities were not the same, the red and the yellow being very appreciably brighter than the blue and the green. In addition to observations with these colors, a piece of black cardboard was introduced in the same position as the glass, thus cutting off the light from that half of the box. This, to preserve the uniformity of the series, is denominated black. The animals were placed in the centre of the box, on the line separating the white from the colored light, and were observed at intervals of one minute for forty minutes, the position of each animal being accurately noted.

At the end of every ten minutes the animals were reset at the centre of the box. The following table gives a summary of the results for each individual. Here again it was impossible to keep the groups constant owing to the death of individuals during the progress of the experiment.

TABLE VI. REACTIONS TO HORIZONTAL COLORED LIGHT

	Group 1											Group 2							
Animals	13	37	41	42	44	33	4	9	43	46	Sum	21	27	31	36	38	52	Sum	Sum Total
Blue	12	9	21	30	18						90	23	2	24	13	37		99	189
White	28	31	19	10	22						110	17	38	16	27	3		101	211
Green	15			9		23	36	17			100	12	19	3	28	40		102	202
White	25			31		17	4	23			100	28	21	37	12			98	198
Yellow	20	10	31	28	19						108	17	11	28	37	35		128	236
White	20	30	9	12	21						92	23	29	12	3	5		72	164
Red	26			9		32	20	17			104	2	30	16	29	36		113	217
White	14			31		8	20	23			96	38	10	24	11	4		87	183
Black		24	27		12				27	26	116	15	18		26	25	5	89	205
White		16	13		28				13	14	84	25	22		14	15	35	111	195

In this table the colored lights are arranged in the order of the spectrum from blue to red. On the hypothesis that blue light has practically the same effect upon animal reactions as white light, while red is about the same as darkness, we might expect that the reactions would be about equally divided between the blue and the white, and that there would be a gradually increasing difference in number as we go down the table, reaching the maximum with the last pair, black-white. This, we see, however, is not quite the case. In both groups the white has a slightly larger number of reactions than the blue, while the pair green-white shows numbers more nearly equal. In the sum totals the yellow shows a greater preponderance over the white than any other color, and the black and white are very nearly equal. Group 1, it is true, shows a fairly regular ascending scale in reactions to the colored lights with the exception of the red, and the same might be said of group 2 if it were not for the very low number of reactions to the black and the exceptionally high showing of the yellow. On the whole, however, the differences are so small and the individual variations are so large that we can only conclude that for these conditions colored light has little or no effect on the reactions of the animals.

In the foregoing experiment the light came from a broad spiral

coil inside the bulb of the lamp, and the distance from it to the edge of the glass was so small compared with the length of the box that there was no sharp dividing-line between the colored light and the white, but rather a wedge-shaped block of lessening saturation of the color, and this wedge, having the point toward the light, took up the whole of the box at the extreme farther end. Thus the imaginary central line dividing the white light from the colored departed farther and farther from the reality as the rear of the box was approached. To obviate this difficulty and to get a check on the previous work, the following series of experiments was undertaken.

b. Reactions to Vertical Colored Light. The same box was used as in the previous experiments, but the end was closed with a black cloth, and an electric light marked 32 c. but measuring only 22 c. was hung exactly over the middle of the box, 40 cm. from the bottom. By means of wires it was arranged that a plate of colored glass could be swung in such a manner that all of one half the box (the whole of one end) was illuminated with the desired color, while the other half was either left white or illuminated with another color. In this way there was a fairly sharp dividing-line between the two colors. The animals were observed at intervals of one minute for 40 minutes, and reset at the middle on the dividing-line every ten minutes as before. Table VII gives the results of the observations.

In this set of experiments it was possible to keep the groups intact except that in group 1 no. 13 had to be replaced by no. 46. If now we conceive the colors arranged in the order of the spectrum with black at one end and white at the other, and consider the black a lower stimulus than the white, we have the ascending series black, red, yellow, green, blue, white. Now since the animals have already been shown to be somewhat negatively phototactic, we should expect them to prefer a color of lower stimulus to one of higher. Turning to the sum totals in the table we find that the first color of each pair (which is always the lower stimulus) has the larger number of reactions in every case but one, the first pair of red-blue. As was stated above, it was impossible to secure colored light of the same intensity by means of the glass at our disposal, and in the present case the red was considerably brighter than the blue. Owing to the fact already mentioned that different intensities of white light seem to have no effect on the reactions it was thought that these differences in the intensities of the colored lights might be overlooked. Since the only thing that could be thought of to account for the anomalous behavior to the red-blue was this difference in intensity, another ex-

periment was undertaken with the same animals under slightly different conditions. A glass aquarium about 40 cm. long by 20 cm. wide was covered with black cardboard and black cloth in such a manner that light could enter only through a space 5 cm. wide at the bottom of each end. Each of these ends was covered, the one with blue, the other with red glass, and 15 cm. from each end was placed an electric light marked 32 c. Later, however, it was found that one of these lamps measured 30 c. and the other 22 c. The red light was found to be much more intense to the eye than the blue, so the former was damped down with tissue paper until the two appeared to have the same intensity. The second pair of red-blue in Table VII gives the results of the observations under these conditions, and these are found to be in harmony with the rest of the table, *i.e.*, the color giving the lower stimulus has the higher number of reactions.

TABLE VII. REACTIONS TO VERTICAL COLORED LIGHT

	Group 1						Group 2						Group 3						Sum
Animal no.	13	37	41	43	44	Sum	21	27	36	38	52	Sum	54	56	58	60	62	Sum	Total
Blue	17	22	14	25	29	107	11	21	26	27	26	111	26	32	26	28	10	122	340
White	23	18	26	15	11	93	29	19	14	13	14	89	14	8	14	12	30	78	260
Green	17	26	11	21	29	104	21	27	30	7	26	111	29	31	26	10	9	105	320
White	23	14	29	19	11	96	19	13	10	33	14	89	11	9	14	30	31	95	280
Yellow	25	21	12	20	35	103	12	20	34	25	29	120	19	20	9	28	22	98	321
White	15	19	28	20	15	97	28	20	6	15	11	80	21	20	31	12	18	102	279
Red	37	33	23	22	28	143	21	30	30	33	22	136	32	37	26	33	28	156	435
White	3	7	17	18	12	57	19	10	10	7	18	64	8	3	14	7	12	44	165
Black	23	20	1	28	34	106	9	12	25	40	32	112	25	32	18	34	16	125	343
White	17	20	39	12	6	94	31	28	15		14	88	15	8	22	6	24	75	257
Black	20	15	13	27	37	112	16	11	16	27	24	94	36	40	18	29	29	152	358
Red	20	25	27	13	3	88	24	29	24	13	16	106	4		22	11	11	48	242
Black	20[1]	25	26	20	32	123	22	15	39	40	32	148	33	22	34	34	11	134	405
Blue	20	15	14	20	8	77	18	25	1		8	52	7	18	6	6	29	66	195
Red	25	17	28	18	8	96	3	15	21	28	20	87	21	13	15	14	28	91	274
Blue	15	23	12	22	32	104	37	25	19	12	20	113	19	27	25	26	12	109	326
Red[2]	21	27	22	10	36	116	13	40	24	21	28	126	22	25	18	21	16	102	344
Blue	19	13	18	30	4	84	27		16	19	12	74	18	15	22	19	24	98	256

[1] No. 46 substituted.

[2] The second pair of red-blue gives the results of an experiment under somewhat different conditions as described above.

The most striking feature of the table is the marked predominance of the red over the white. Here the red reaches 73 % of the total number of reactions, and inspection shows that this predominance is uniform not only through the groups but even for the individuals. The constancy of this reaction and the fact that it is so much more frequent than the one to the black as compared with the white, would lead one to expect that the red would have the higher percentage in the combination black-red. Such, however, is not found to be the case, although it does happen with one group. If the arrangement of our color-scale in accordance with increasing intensity of stimulus were correct, we should expect a gradually increasing predominance in reactions to colored light over those to white in the first five pairs. Instead of this we find that green and yellow stand nearest to the white, blue and black come next and are almost equal, while red is very much higher than any. In the pairs black-red and black-blue the red holds its predominance over the blue at about the same rate as in the second pair of the direct comparison, red-blue. The wide individual variations, however, in all these reactions to colored light, except perhaps in the case of red-white, indicate that there is nothing very regular, stereotyped, or mechanical about them. The most that can be said is that in a general way the red end of the spectrum furnishes a less intense stimulus to negative reaction than the blue.

A tendency to habit formation was noticed during the course of these experiments, and it is possible that this may have influenced the results somewhat. Many individuals apparently formed a habit of going to a certain corner as soon as they were reset at the centre. The positions in which they were set were varied and they were headed in different directions, but within a minute after they were released in the middle of the box they would be found in their favorite corner. This was especially the case with no. 38 in Table VI, and I think accounts in some measure for the persistent avoidance of the white. In no case did this continue throughout the whole series, but would sometimes be noted for two or three days at a time in the case of an individual. What were the controlling factors in this habit formation, the means by which orientation and recognition were effected, I was unable to determine.

3. *Reactions to Objects*

In no case did an animal give any sign of perceiving stationary objects in its path or of avoiding them in any way that could be referred to a visual stimulus. When the animal approached an obstruction

there was no hesitation in the movement until the object was touched. Usually even when the antenna had touched the object the animal did not stop, but continued until the contact of the chelæ or even of the rostrum made further movement in that direction impossible.

With moving objects the case was quite different. Here the condition and disposition of the individual animal seemed to be the deciding factors. Often when the animals were trying to climb out of a shallow pan in which they were kept in the experimenting-room, raising a finger or holding out a pencil would be sufficient to make them stop or even start back into the pan. Nor was this response occasioned by any change in the intensity of light, such as that caused by a shadow falling on the animal, for they would react to a movement made on the opposite side of them from the window. In fact, no. 56, the most active in response to moving objects, seemed to react more vigorously to a motion made on the opposite side than when it was made between him and the light. Whenever a person came near the aquarium he and one or two others would take an attitude of defence, and would "face about" to correspond to any movement the person made toward one side or the other. When in the pan mentioned above, any movement of a person within two or three yards of him usually called forth a reaction on his part, and if the pan were placed on the table and the person moved slowly round it, the animal turned with the person, making a complete circuit of the pan.

Reaction to a smaller moving object, however, was not so marked. A black object, $20 \times 8 \times 8$ cm., was suspended above the middle of the pan so that if set swinging it would just pass over the top. When it was pulled to one side the animal responded slightly, but after the first swing he seemed to pay no more attention to it. When the operator stepped out from behind the screen, the animal was as keen in its response as before. The experiment was now tried of allowing the object to approach from one direction while the operator moved to a position at right angles to its line of movement. Without hesitation the animal moved so as to keep fronting the operator, without paying any attention to the movement of the smaller object, although this was much nearer.

These observations on the reactions of the crayfish to stationary and moving objects are in line with the conclusions of Plateau[1] and Exner[2] drawn from observations on other Arthropods. It is Exner's

[1] Recherches expérimentales sur la vision chez les Arthropodes, Mém. Corronnés de l'Acad. Roy. des Sci. etc. de Belgique, vol. 43, pp. 1–91, 1889.

[2] Die Physiologie der facettirten Augen von Krebsen und Insekten, 1891.

belief that the compound eye is a visual apparatus which is almost worthless for detecting the *forms* of objects, especially if these objects are stationary, but that it may furnish a very keen perception of *moving* objects.

II. Experiments with Sounds

Hensen[1] stated that Palæmon and Mysis reacted to sounds made by striking a thin, resonant board floating on the surface of the water, or by tapping the walls of the aquarium or of the room. Beer[2] repeated Hensen's experiments, but denied that the Crustacea reacted to sounds, and claimed that their movements were due to visual and tactual stimuli. Prentiss[3] confirmed Beer's results on Palæmonetes, and noted that the reactions were only slightly diminished by the removal of the otocysts, but that removal of the antennæ and antennules caused their almost complete cessation. More extended experiments were made on the fiddler crab, *Gelasimus pugilator*, which is on land a good deal of the time, and Prentiss's conclusions are: "(1) The reactions formerly attributed to sound-stimuli are nothing more than tactile reflexes. (2) The otocyst has little or no part in calling forth these reactions. (3) There is no direct evidence to prove that decapod Crustacea hear, and until such evidence has been obtained, we are not warranted in ascribing to the otocyst a true auditory function."

The experiments performed on the crayfish in this connection all resulted negatively and go to confirm Beer's and Prentiss's conclusions. Rapping upon a board floating in the water, and tapping the sides of the aquarium did not cause the slightest apparent reaction in the animals under observation, even though the vibration of the water could be plainly perceived by the sense of touch in the hand. When a rather large electric bell was sounded just over the surface of the water some reactions were observed which were evidently due to the movements of the hammer, but there was nothing which could be referred to the sound-stimuli. If the bell was held against the sides of the aquarium, or in the water near the animals, the vibration could

[1] Studien über das Gehörorgan der Dekapoden, Zeitsch. f. wiss. Zool., vol. 13, pp. 319–412, 1863.

[2] Vergleichend-physiologische Studien zur Statocysten-function, I. Ueber den angeblichen Gehörsinn und das angebliche Gehörorgan der Crustaceen, Arch. f. d. ges. Physiol., vol. 73, pp. 1–49, 1898; Idem. II. Versuche an Crustaceen, Arch. f. d. ges. Physiol., vol. 74, pp. 364–382, 1899.

[3] The Otocyst of Decapod Crustacea, Bull. Mus. Comp. Zoöl. Harv. Univ., vol. 36, pp. 165–251, 1901. (Contributions, no. 123.)

be plainly felt by the fingers, yet no reactions on the part of the crayfish were observed. A metal snapper making a crack like a small pistol-shot was tried both in and out of the water but with no success in producing a reaction. A large hand tuning-fork, when held with its base pressed firmly against the glass walls of the aquarium, gave a deep rich tone of great volume, or when lightly touched to the glass produced a shrill, piercing, penetrating sound which was extremely sharp and disagreeable. Here again the vibrations of the water were quite perceptible to the hand at a distance of 10 cm., yet in neither case was there a sign of a reaction. Finally two electric tuning-forks, one of 256, the other of 512 vibrations per second, were tried on the animals taken one by one, and especial attention was given to the regular movement of the little thread-like appendages which keep up the current of water to the gills, with the idea that perhaps their rate of movement might be affected. In no case was there the slightest movement that could be referred to vibration, although here again the tactile stimulus was very perceptible to the finger. None of these experiments, then, give any indication that the crayfish reacts to vibratory stimuli which to the human ear produce sound.

III. Rotation Experiments

It has been found that the higher vertebrates, on being rotated on a turn-table, exhibit all the symptoms which accompany the sensation of dizziness in man. The question arises, to what degree and in what manner do invertebrates respond to rotation? Schaefer,[1] the first to take up this question, denied on rather meagre observations that Crustacea respond in any way to rotation on the turn-table. Kreidl[2] showed that this statement was altogether too sweeping, that Palæmon reacts very definitely to rotation by running in the opposite direction. Bunting[3] tried the crayfish, but all the rotation experiments resulted negatively, so she was led to confirm Schaefer's statement so far as the crayfish is concerned. Bethe[4] found that Carcinus behaved in a very definite manner on being rotated, that during the rotation

[1] Das Verhalten wirbelloser Thiere auf der Drehscheibe, Zeitsch. f. Psych. und Physiol. d. Sinnesorgane, vol. 3, pp. 185–192, 1892.

[2] Weitere Beiträge zur Physiologie des Ohrenlabyrinthes, II. Mittheilung, Versuche an Krebsen, Sitzungsb. Kais. Akad. Wiss., Wien., vol. 102 (Part. 3), pp. 149–174, 1893.

[3] Ueber die Bedeutung der Otolithenorgane für die geotropischen Functionen von Astacus fluviatilis, Arch. f. d. ges. Physiol., vol. 54, pp. 531–537, 1893.

[4] Das Nervensystem von Carcinus mænas, I. Arch. f. mikros. Anat., vol. 50, pp. 460–547, 589–640, 1897.

the animals ran in the opposite direction to that in which they were turned, and as soon as the motion ceased they began running in the other direction. Finally Lyon,[1] while agreeing with Bunting that adult crayfish do not react to rotation, discovered that young animals two or three centimetres long react very prettily to the movement, going in a direction opposite to the turn. To confirm and if possible extend these observations on the crayfish was the purpose of the following experiments.

It was soon found that a great deal depended on the method of experimentation. None of the experimenters mentioned above gives any detailed description of the manner in which the experiments were carried out. One is left uncertain whether the animals were placed on the periphery of the turn-table or over the centre, whether in the former case they were set with their heads toward the centre or away from it, or placed at right angles to a radius, or whether they were merely set down in any chance fashion and whirled about. The same indefiniteness exists in most of the accounts as to how fast they were turned, and whether the experiments were performed in the air or in the water. Finally it is not stated whether the rotation was always in the same direction, or whether its direction was alternated.

The turn-table used in the following experiments was one that had to be turned by hand, so that it was impossible to regulate the speed accurately. The crank, however, was not attached directly to the rotating board, but was connected with it by means of a gearing so that one turn of the crank produced about ten turns of the table. This gearing gave a steadying effect to the motion so that the speed could be kept tolerably constant. A circular pan, about 15 cm. in diameter at the bottom with the sides slightly sloping outward, was set so that its centre coincided with the axis of the rotating table. It was in this pan that all the experiments were tried. Through various preliminary experiments to determine the most favorable speed, it was found that a rotation rate of over one turn of the table per second produced such a strong centrifugal force that unless the animals were set exactly over the centre they were swept off against the side of the pan in such a manner that it was difficult to decide whether the rotation as such had any effect upon their movements. It was finally decided that the best results were obtained from a rate of approximately one rotation in two seconds.

[1] Contribution to the Comparative Physiology of Compensatory Movements, Amer. Jour. of Physiol., vol. 3, pp. 86–114, 1899.

It soon became evident that when the larger and more sluggish crayfish were merely dropped in the pan and rotated there was no particular reaction. This was true whether the animals rotated were in the air or in the water. The smaller and more active crayfish, however, showed a decided tendency to run either with or against the direction of the rotation, especially when the experiments were carried on in the water. In no case was there any tendency to go in the opposite direction when the rotation ceased, except in so far as the animals were carried along by the water. To get a quantitative expression for these tendencies a more delicate method of experimentation was resorted to. If there was a tendency on the part of the active animal to move either with or against the rotation, such a tendency might also be supposed to exist in the sluggish animal, only in the latter the inertia was sufficiently strong to prevent its appearance. If, however, the animals should be set radially to the periphery of the pan, the tendency to go with or against the rotation would be exhibited in the direction in which they turned out of the radial position. For it was found that no animal would remain in that position for any great length of time. Two groups of animals were used for these experiments, five animals in each group, and the first group was selected from the smallest and most active animals, the second from the largest and most sluggish. Each animal was set in two positions, position I, with the head toward the centre, position II, with the head away from the centre. Each animal was given ten trials in each position, and the number of times it turned in a direction *with* the rotation is set down in the + column, the number of times it turned *against* the rotation is indicated in the — column. In general from 5 to 15 turns were necessary for the orientation of the animal, though sometimes the number ran up to 30 or 40. Each trial was made in the opposite direction to the preceding one, in order to avoid the formation of any habit in turning. All these experiments were carried out in water, the depth of which in the pan was about 4 cm. In order that there should be no difference between the velocity of the water and that of the pan, the table was rotated a few times before the animal was put in. As a check a series of experiments of 5 in each position was performed in the air on the more active group.

The following table shows the results of these experiments in rotation:

TABLE VIII. REACTIONS TO ROTATION

	In Water						In Air							
	I		II		Sum		I		II		Sum		Sum Total	
Group 1	+	−	+	−	+	−	+	−	+	−	+	−	+	−
44	5	5		10	5	15	1	4		5	1	9	6	24
49	2	8	4	6	6	14		5	1	4	1	9	7	23
56	3	7	8	2	11	9	5		3	2	8	2	19	11
62	4	6	10		14	6	2	3	2	3	4	6	18	12
64	2	8	9	1	11	9	1	4	1	4	2	8	13	17
Sum	16	34	31	19	47	53	9	16	7	18	16	34	63	87
Group 2														
21	5	5	3	7	8	12								
27	2	8	5	5	7	13								
36	7	3	8	2	15	5								
37	2	8	3	7	5	15								
54	5	5	6	4	11	9								
Sum	21	29	25	25	46	54							46	54
Sum Total	37	63	56	44	93	107	9	16	7	18	16	34	109	141

Examination of the table reveals great individual variation. Some animals, as nos. 44, 49, and 37, turn rather constantly against the direction of the rotation, while others, as nos. 56 and 36, are almost as constant in their movement with the rotation. On the whole we observe that for each group, and for Group 1 in both water and air, there is a slightly greater tendency to go against the rotation than with it. This tendency, strange to say, comes out much more clearly in the air than in the water. It is evident, however, from the variation exhibited that there is nothing very stereotyped or mechanical about the reaction. Mention should be made of the fact that usually (though not always) the animals not only oriented themselves with reference to the rotation, but moved forward in that direction as long as the rotation continued.

IV. GEOTAXIS, BAROTAXIS, AND TURNING

(1) *Geotaxis.* So far as my knowledge extends, no experimental work has been done to determine the geotaxis of decapod Crustacea. Most of the vertebrates are positively geotactic, while a great many of the invertebrates, particularly unicellular organisms, larvæ of moths and butterflies, slugs, etc., are negatively geotactic. Parker [1] found that in the case of the Copepod, *Labidocera æstiva*, the females exhibited strong, the males weak, negative geotaxis. In the investigation of the geotaxis of the crayfish, two sets of experiments were undertaken. In the first the method of procedure was as follows:

[1] The Reactions of Copepods to Various Stimuli, Bull. U. S. Fish Comm., vol. 21, pp. 103–123, 1902.

On a level table before a window a board was so arranged that it could be set at an inclination of 5°, 10°, 15°, 20°, and 25° either toward or away from the window. Starting, let us say, with the inclination toward the window, each one of a group of five animals was placed on the board with the right side to the window five times. The board was then inclined the same amount away from the window and the process was repeated. The same procedure was carried out with the animals set with the left side to the window. The following table gives the results of this set of experiments.

TABLE IX. GEOTAXIS IN FRONT OF WINDOW

	10			12			14			16			18			Totals		
	+	−	±	+	−	±	+	−	±	+	−	±	+	−	±	+	−	±
5°	14	5	1	11	7	2	11	9		13	7		8	10	2	57	38	5
10°	13	7		12	7	1	13	6	1	16	4		14	6		68	30	2
15°	17	3		14	6		16	3	1	17	3		9	11		73	26	1
20°	12	8		17	3		18	1	1	19	1		14	6		80	19	1
25°	18	2		19	1		19	1		17	3		16	4		89	11	

From this table it appears that the crayfish is positively geotactic, and that the positive geotaxis increases regularly with the increase in inclination. As a check on these results another set of experiments was undertaken with different animals under different conditions. The board was placed on a level table in the centre of a darkened room, and the operator stood behind a screen so as to be quite hidden from the animals. In order to observe the orientation a 2 c. incandescent electric light was suspended directly above the spot where the animals were set, at a distance of 60 cm. above the board. Each animal of a group of five was set five times in each of four positions, viz., head down the incline, head up the incline, and at right angles to it with first the right and then the left side down the slope. The results were as follows:

TABLE X. GEOTAXIS IN DARKENED ROOM

	41			46			48			51			64			Totals		
	+	−	±	+	−	±	+	−	±	+	−	±	+	−	±	+	−	±
5°	12	4	4	13	4	3	8	10	2	14	5	1	11	7	2	58	30	12
10°	14	5	1	13	6	1	11	8	1	13	6	1	14	3	3	65	28	7
15°	16	4		15	5		13	7		11	6	3	14	2	4	69	24	7
20°	16	4		19	1		16	4		20			17	2	1	88	11	1

It will be observed that Tables IX and X agree quite well in the main, and we may conclude that the crayfish is positively geotactic and that the positive reactions vary from 58 % at 5° to 89 % at 25°.

(2) *Barotaxis.* Verworn[1] uses the term barotaxis in an inclusive

[1] General Physiology, English translation by Frederic S. Lee, 1899.

sense to cover all pressure phenomena that can be classed under the sub-heads of thigmotaxis, rheotaxis, and geotaxis. It seems preferable to me to employ the term in a more restricted sense of reaction to pressure other than the pull of gravity, the flow of a current, or the contact with bodies. The following experiment with the crayfish furnishes us, I think, with a case in point.

A glass aquarium, 54 cm. long and 28 cm. wide, was so inclined that the water was 20 cm. deep in one end and 8 cm. deep in the other. A board was so anchored that one end rested on the bottom at the shallow end of the aquarium while the other end projected slightly out of and above the deepest water. The board was about 45 cm. long, so that its slope was very gradual. Nine animals were placed in this aquarium and observed for three successive days. If we denote the bottom of the deep end of the aquarium by A, the shallow end under the board by B, the shallow end on top of the board by C, and the end of the board at the surface of the water by D, the results of the observations were as follows: On the first day 1 animal was found at D, 6 at C, and 2 at B. On the second day 5 were at C, 3 at B, and 1 at A. On the third day 1 was at D, 4 at C, and 4 at B. Totals, 2 at D, 15 at C, 9 at B, and 1 at A.

While these observations were too few to base very positive statements on, the striking fact that only one animal was found at A, the deep end of the aquarium, whereas 15 were noted on top of the board at C, indicates strongly that the animals avoid the deeper water. That the animals were found *on top* of the board, not under it, indicates that the observation is not to be referred to thigmotaxis, although the latter is doubtless very strong, as we shall see later. It should be observed that the negative barotaxis works against and overcomes the marked positive geotaxis which, as we have just seen, the animals exhibit in the air. Under the influence of the positive geotaxis, we should expect to find the greater number of the animals at A, — a condition which is speedily realized if we let the water run out of the aquarium. We conclude, therefore, that at certain pressures (specifically at the pressure exerted by water at a depth of 20 cm.) the crayfish is negatively barotactic.

(3) *Turning.* In the experiments with light it was observed that very seldom do the animals, when placed upon a surface, move off at once in a straight line, but usually they first turn through an angle of 90° or more and then start off straight. This came out strongly in the work on geotaxis, where oftentimes, when the animal was set with the head up the incline, the reactions would be preponderantly pos-

itive, whereas when set with the head down the incline the reactions were on the whole negative. In other words, when headed up the incline the animal would go down, and when headed down he would more often go up. Some experiments were tried under various conditions to determine how general this tendency is. The table presents the results in condensed form.

TABLE XI. EXPERIMENTS IN TURNING

Nos.		10	12	14	16	18	Sum		7	9	15	17	25	Sum	
	I							III							
90°−		8	5	6	8	10	37		2	4	7	5	5	23	
90°		4	6	7	2	2	21		3	1				4	
90°+		8	9	7	10	8	42		5	5	3	5	5	23	
	II							IV							
90°		2	1	2		2	7			9	3	1	2	15	
90°		1		1		2	4		3	5	3	3	1	16	
90°+		7	9	7	10	6	39		12	1	9	11	11	44	
Nos.		41	43	46	48	64	Sum								Sum Totals
	V														
90°−		9	11	12	14	2	48								130
90°		2	1	2	1	3	9								54
90°+		9	8	6	5	15	43								191

In this table the first line indicates the number of times each animal turned less than 90° when starting off from the position in which it was set, the second line the number of times the amount of turn was practically 90°, and the third more than 90°. The five parts of the table mark the different conditions; in Part I twenty observations were made on each animal placed on a level board before the window, and set now with the right now with the left side toward the window. In Parts II and III the animals were set with the head turned now toward now away from the window. In Parts IV and V the animals were placed on a level board in the middle of a darkened room with a 2 c. light about three feet above them. This was to exclude any possible directive influence of light. In all cases the operator was concealed by a screen. In Parts I and V twenty observations were made on each animal, in II and III ten, and in IV fifteen.

Rarely the animal would turn completely round and start off in the direction originally set, but usually the turn was between 90° and 180°. When once the animal began to move off, it would ordinarily keep to an approximately straight line. How seldom this was observed when the animals were first set down may be judged from the fact that out of a total of 375 observations in only 18 did the animals move straight ahead from their original position. From the table we observe that in over 65 % of the cases (a proportion of almost

two to one) the animals turned through 90° or more before starting off. At present the writer has no explanation to offer for this phenomenon.

V. Thigmotaxis and Touch Reactions

(1) *Thigmotaxis.* Experiment has shown that there are some animals which tend to avoid contact with objects as much as possible, and on the other hand there are animals that seek to get as much of the surface of their bodies as possible in contact with objects. The former are spoken of as negatively, the latter as positively thigmotactic. Does the crayfish show any tendency in the one way or the other, and if so is it positively or negatively thigmotactic? In a large glass aquarium, 80 cm. long and 40 cm. wide, was a thin wooden box, 22 cm. long and 16 cm. wide, set in one corner 4 cm. from the glass walls. At the bottom of the box was an opening where the crayfish could enter. The following table shows the disposition of the animals for 27 different days, on which one examination was made each day. Line I indicates the number of times each animal was found against the walls of the aquarium, line II in the 4 cm. space between the box and the walls of the aquarium, line III inside the box against its sides, and line IV resting freely in the middle of the aquarium or in the middle of the box.

Table XII. Thigmotaxis Reactions

Animal	4	5	9	13	21	27	29	31	33	35	36	37	38	39	41	42	43	44	45	46	47	48
I	4	9	2	10	4	17	16	6		6	10	3		2	11	9	3	2	1	9	1	15
II		5	2	13	18	9		5	2	4	5	22	1	14	11	3	6	11	1	9	4	7
III		2	1	2	5	1	9	3	2	3	11	2	24	7	1		4	14		9	1	5
IV	1	11					2		1				2	4	4	1	2					

Animal	49	51	52	54	56	58	60	62	64	Sum Totals
I	6	11	3	1	2	4	7	10	12	195
II	8	3	7	5	8	1	3	2	1	190
III	1		16	9	5	10	3	2	2	154
IV		1	1				2	1		33
										572

In order to appreciate the significance of the figures in this table it is necessary to consider the amount of lateral surface with which it was possible to come in contact in each case. In IV of course it was zero, in III it was 76 cm. with four corners in close proximity, in II it was only 38 cm. and one corner, but the space was so narrow that there was practically a contact-surface on both sides, and in I there was 212 cm. of lateral surface with three corners. I mention

corners in this connection because they were almost invariably occupied. If we examine the table with these facts in mind, we find, (1) that the number of animals resting freely without contact with any lateral surface is very small, only about 6% of the whole; (2) that the number of animals found in the narrow space between the box and the walls of the aquarium is very large in proportion to the length of the space: indeed the animals were frequently found wedged into this space three or four deep; (3) that the number of animals found in the box was probably due largely to the fact that they found in it a greater lateral contact-surface, particularly in the corners, than was possible outside.

Two or three minor considerations are of interest. The animals were frequently observed "on edge" about half out of the water, that is, with the ventral surface of the body pressed against the vertical surface against which they were resting. This was also observed where the water was so deep that none of the members could touch the bottom. It was perhaps on account of the quality of the surface affording a rougher contact that so large a number of the animals were found in contact with the wooden box rather than the smooth, slippery surface of the glass. In the centre of the aquarium a wooden stopper 2 cm. in diameter projected about 15 cm. above the surface of the water. Very often a crayfish would be found almost at the top of this stopper, completely out of the water. This tendency to climb was frequently observed in the light-reaction experiments, where the animals would climb up on any piece of wood that chanced to be left in the box. It reminds one of the tree-climbing crabs of the West and East Indies. Along the creeks of Ohio I have frequently seen crayfish that had climbed up on logs or sticks that projected some feet out of the water.

In the table we see decided evidences of "habit" in the sense of an animal returning to the same place which it had occupied. No. 5 has almost half the observations in the open, nos. 21, 37, and 39 showed a decided preference for the space between the box and the aquarium wall, while nos. 38, 44, and 52 were more frequently found on the inside of the box. This recurrence to a particular position also came out in the light-reaction work, where an individual would return to the same spot in the box for days at a time as soon as released.

From the above considerations we conclude that the crayfish is strongly positively thigmotactic and that this thigmotaxis probably plays a most important part in the life of the animal.

(2) *Touch Reactions.* Lemoine[1] investigated the reactions of crayfish to touch-stimuli and found that the plates of the telson, the sternal portions of the thorax, the abdominal pleopods, the chelæ, and particularly the antennæ toward their points are especially sensitive, but that nowhere, even on the back of the carapace, is a touch-stimulus altogether devoid of reaction. Gulland[2] found that a needle could be inserted between the tufts of setæ on the chelæ without causing any reaction, but as soon as one of the hairs was touched, the chela closed with a snap. Considering the setæ as the organs of touch, he claimed to have found that the eyes, eye-stalks, and carapace (which he says have no setæ) are impervious to tactile impressions. This claim of Gulland's is strangely at variance with the facts. In no case have I been able to bring about retraction of the eye-stalk by visual stimulation, but a very light touch-stimulus on the eye itself or on the eye-stalk or a stronger stimulus on some portion of the head will cause the eye to be drawn in. It is true that after repeated stimulation the eye is retracted no longer, and with a heavy bristle one can make a perceptible indentation in the corneal surface without the eye being withdrawn.

The antennæ, from their anatomical structure, their position, and the manner in which they are carried, are generally considered the special organs of touch. Nevertheless, as far as the reactions of the animal are concerned, a stimulation of the antennæ by touch produces a less decided response than almost any other portion of the body. If the stimulus is very light no reaction at all is observed in most cases, and if stronger the antennæ are moved away, but that is all. A stimulation of the edge of the telson produces a more decided reaction. Either the animal folds it under the abdomen at once or faces about like a flash in an attitude of defence; frequently both reactions occur. While the response to stimulation of the chelæ was decided, that to touch on the first chelipedes was quicker and more accurate. The mouth-parts are also very sensitive to touch. I cannot agree with Gulland's assertions as to the insensitiveness of the carapace, for I have been able to find no place upon it where a light-stimulation would not produce a reaction. In this connection a curious phenomenon is characteristic of the animal. If the carapace or the front portion of

[1] Recherches pour servir à l'histoire des systèmes nerveux, musculaire, et glandulaire de l'écrevisse, Ann. des Sci. Nat., series 5, vol. 9, pp. 99–280; vol. 10, pp. 5–54, 1868.

[2] The Sense of Touch in Astacus, Proc. Roy. Physiol. Soc. Edinburgh, vol. 9, pp. 151–179, 1886.

the abdomen be lightly stroked with a solid object such as a pencil, the animal will slowly turn toward the stimulus on its antero-posterior axis. If, now, a like stimulus be applied on the other side, the animal will roll back through the normal position to a like inclination toward the stimulus on the other side. If the alternation be kept up and the change made quickly, a continuous and curious rolling movement is maintained, the animal growing more and more excited until it scampers off with a kind of cramp-like motion. With some animals this rolling reflex is more marked than with others, but in no case is it altogether lacking. Some animals have been known to roll so far over that they topple over on their backs. Dr. Yerkes informs me that he has observed the same phenomenon in a less degree with turtles when the edge of their shell is stimulated by scratching. The movement seems to be caused by the reflex stimulation of the extensor muscles on the opposite side of the body from the part stimulated. The thrust of the legs thereby brought about raises that side of the body and thus causes a rotation to some extent about the antero-posterior axis. But how was this connection between the stimulation of one side of the body and the contraction of the extensor muscles of the other side established? I have no doubt that it is intimately connected with the positive thigmotaxis described above. These animals live under loose stones for the most part, and thus the carapace gets a great deal of stimulation. If the animal is stimulated on one side, a contraction of the extensor muscles of the opposite side tends to roll the animal toward the source of the stimulus, and hence to increase the contact. In the race-history of the animal this has doubtless been advantageous in enabling it to escape the dangers of its habitat.

In a succeeding paper the writer hopes to discuss the reactions of the crayfish to chemical stimuli. In conclusion he desires to make acknowledgment to Dr. Robert M. Yerkes of the Harvard Psychological Laboratory for kindly suggestions and helpful criticism throughout the course of the investigation.

Summary

(1) Crayfish are somewhat negatively phototactic, going away from rather than toward the source of light in the ratio of 62 % to 38 %. The different intensities employed in this investigation produced very little difference in the reactions. The average reaction-time was much less for the group of animals which showed the highest

percentage of negative reactions, indicating a greater general sensitiveness. Variations of the position in which the animals were set affected the results very slightly.

(2) Previous confinement in the dark tended to increase slightly the number of negative reactions, and previous exposure to strong light tended to decrease the number, but the results were not constant. An increase in temperature tended to decrease the number of negative reactions to light, but here again the results were somewhat conflicting.

(3) Reactions to horizontal colored light showed a tendency to go to the colored light rather than to the white in the following order: Blue 47 %, green 50.5 %, black (or the absence of light) 51 %, red 54 %, yellow 59 %. In the case of vertical colored light the comparison with the white resulted somewhat differently, as follows: Green 53 %, yellow 53.5 %, blue 57 %, black 57 %, red 72.5 %. In the latter experiments the animals showed a marked and constant preference for the red.

(4) The animals showed no signs of reaction to static objects from visual stimulation, *i. e.*, there is no evidence of visual perception of form in the case of stationary objects. Moving objects, especially large ones, are plainly perceived and definitely reacted to.

(5) There were no reactions whatever caused by those vibrations which to the human ear produce sound. So far as these experiments go, the animals cannot be said to hear.

(6) In rotation experiments individual animals were rather constant in moving either with or against the direction of the rotation, but no definite tendency for all animals was observed.

(7) The pull of gravity was followed with constantly increasing frequency from 58 % at 5° to 89 % at 25°. Therefore we conclude that the animals are positively geotactic. They are negatively barotactic, avoiding the pressure of water at the depth of 20 cm., and this is sufficient to overcome their positive geotaxis. When placed upon a level surface the animals show a peculiar tendency to turn through a greater or less angle before starting out in a straight line. In only 18 out of 375 observations, or 5 %, did the animals start straight, in 30 % they turned through an angle of less than 90°, and in 65 % they turned through an angle of 90° or more.

(8) The crayfish is positively thigmotactic in a marked degree, as is indicated by the fact that in only 33 out of 572 observations, or less than 6 %, were the animals found resting in the open, while in 190, or 33 %, they were found in a narrow opening between two vertical surfaces.

(9) The animal is sensitive to touch over the whole surface of the body, but especially on the chelæ and chelipedes, the mouth-parts, the ventral surface of the abdomen, and the edge of the telson. If one side of the carapace or of the dorsal surface of the abdomen be stimulated, the extensors of the legs on the opposite side are contracted, and the animal turns on its antero-posterior axis toward the source of the stimulus. If opposite sides be stimulated alternately, a peculiar rolling motion is set up.

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